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ARE THERE QUANTUM JUMPS?

PART I*

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'. . . cominciai a credere, che uno, che lascia un'opinione imbevuta col latte, e seguita da infiniti, per venire in un' altra da pochissimi seguita, e negata da tutte le scuole, e che veramente sembra un paradosso grandissimo, bisognasse per necessità, che fusse mosso, per non dir forzato, da ragioni più efficaci.' Galileo, *Dialogue on the Two Greatest World Systems*, 2nd Day.

1 *The Cultural Background*

PHYSICAL science, which aims not only at devising fascinating new experiments, but at obtaining a rational understanding of the results of observations, incurs at present, so I believe, the grave danger ot getting severed from its historical background. The innovations of thought in the last 50 years, great and momentous and unavoidable as they were, are usually overrated compared with those of the preceding century; and the disproportionate foreshortening by time-perspective, of previous achievements on which all our enlightenment in modern times depends, reaches a disconcerting degree according as earlier and earlier centuries are considered. Along with this disregard for historical linkage there is a tendency to forget that all science is bound up with human culture in general, and that scientific findings, even those which at the moment appear the most advanced and esoteric and difficult to grasp, are meaningless outside their cultural context. A theoretical science, unaware that those of its constructs considered relevant and momentous are destined eventually to be

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framed in concepts and words that have a grip on the educated community and become part and parcel of the general world picture—a theoretical science, I say, where this is forgotten, and where the initiated continue musing to each other in terms that are, at best, understood by a small group of close fellow travellers, will necessarily be cut off from the rest of cultural mankind ; in the long run it is bound to atrophy and ossify, however virulently esoteric chat may continue within its joyfully isolated groups of experts. This has happened before in similar circumstances. Benjamin Farrington puts it admirably in his *Greek Science*,[1] vol. 2, p. 173 :

> ' Perhaps the most decisive defeat of the scientific spirit in antiquity had been the loss of the sense of history. History is the most fundamental science, for there is no human knowledge which cannot lose its scientific character when men forget the conditions under which it originated, the questions which it answered, and the functions it was created to serve. A great part of the mysticism and superstition of educated men consists of knowledge which has broken loose from its historical moorings.'

The disregard for historical connectedness, nay the pride of embarking on new ways of thought, of production and of action, the keen endeavour of shaking off, as it were, the indebtedness to our predecessors, are no doubt a general trend of our time. In the fine arts we notice strong currents quite obviously informed by this vein ; we witness its results in modern painting, sculpture, architecture, music and poetry. There are many who look upon this as a new buoyant rise, while others regard it as a flaring up that inaugurates decay. It is not here the place to dwell on this question, and my personal views on it might interest nobody. But I may say that whenever this trend enters science, it ought to be opposed. There obviously is a certain danger of its intruding into science in general, which is not an isolated enterprise of the human spirit, but grows on the same historic soil as the others and participates in the mood of the age. There is, however, so I believe, no other nearly so blatant example of this happening as the theories of physical science in our time. I believe that we are here facing a development which is the precise counterpart of that in the fine arts alluded to above. The most appropriate expression to use for it is one borrowed from the history of poetry : Góngorism. It refers to the poetry of the Spaniard Luis de Góngora (1561-1627), very fine poems, by the way, especially the early ones. Yet also his later poems (to which the term more particularly refers) are well sounding and *they all make sense.* But he uses all his acuity and skill on making it as difficult as possible to the reader to unravel the sense, so that even natives of Castile use extended commentaries to grasp the meaning safely.

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[1] Pelican Books, London, 1949

One ought not, I think, to say that if, in this, physics is following a general trend of our time, we must not oppose it. Though we are entirely the product of historical development, yet it is we who make its continuation and not history that drags us along a predestined trail. It depends entirely on us, on our stopping to think and acting according to reason, whether there will be decay or a new rise after the crisis. This is what Bertrand Russell in recent years has not tired to inculcate with regard to much more momentous questions than the fate of theoretical physics. However, here we shall be concerned with the latter.

My friend and scientific colleague Professor Hans Thirring, in his book *Homo Sapiens*,[1] in which he conducts an elaborate and very creditable campaign against War, and for Universal Peace, incidentally opines that in antiquity everybody except a few men of genius considered the earth to be a flat disk. Professor E. P. Wigner, in an article on ' The Limits of Science '[2] is in doubt whether to date the ' birthyear ' of chemistry around 1780 (Lavoisier) or at 1808 (Dalton's law). Physics, he says, is somewhat older, since Newton's *Principia* became available in 1687. He grants that ' Archimedes discovered laws of physics around 250 B.C. but his discoveries can hardly be called the real beginning of physics.' I must not take up space by refuting these strange views, but refer the reader to Professor Benjamin Farrington's two excellent Pelican books on *Greek Science.* Still I would mention that among the ' insignificant ' discoveries of that period was the inference, drawn (probably by Archimedes) from the heliocentric system of Aristarchus, that the fixed stars must be at least at a distance of, in our units, about two light years ; and the further conclusion that from there the sun would appear as a faint star, and therefore, inversely, many of those stars must equal and even exceed the sun in size—or luminosity, as we would call it today. Of course scientific knowledge takes some time to get a grip on the cultured community. Charles Darwin tells us in the *Voyage of a Naturalist* of the sensation he caused in 1833 among the

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[1] Wien, 1948

[2] *Proc. Am. Philosoph. Soc.* 1950, **94**, 422

'educated' society in Argentine by telling them that the earth is a sphere. This knowledge was then more than 2,300 years old.

What has all this to do with quantum jumps? I have been trying to produce a mood that makes one wonder what parts of contemporary science will still be of interest to others than historians 2,000 years hence. There have been ingenious constructs of the human mind that gave an exceedingly accurate description of observed facts and have yet lost all interest except to historians. I am thinking of the theory of epicycles. I confess to the heretical view that their modern counterpart in physical theory are the quantum jumps. Or rather these correspond to the *circles* which the sun, the moon and the stars were thought to describe around the earth in 24 hours after earlier and better knowledge had been condemned. I am reminded of *epicycles* of various orders when I am told of the hierarchy of *virtual* quantum transitions. But let these rude remarks not deter you. We shall now come to grips with the subject proper.

2 *The Discontinuous States as Proper Modes*

Max Planck's essential step in 1900, amounted, as we say now, to laying the foundation of quantum theory; it was his discovery, by abstract thought, of a *discontinuity* where it was least expected, namely in the exchange of energy between an elementary material system (atom or molecule) and the radiation of light and heat. He was at first very reluctant to draw the much more incisive conclusion that each atom or molecule had only to choose between a *discrete* set of 'states'; that it could normally only harbour certain discrete amounts of energy, sharply defined and characteristic of its nature; that it would normally find itself on one of these 'energy levels' (as the modern expression runs)—except when it changes over more or less abruptly from one to another, radiating its surplus energy to the surrounding, or absorbing the required amount from there, as the case may be. Planck was even more hesitant to adopt the view that radiation itself be divided up into portions or light-quanta or 'photons,' to use the present terminology. In all this his hesitance had good reasons. Yet only a few years later (1905) Einstein advanced the hypothesis of light-quanta, clinching it with irresistible arguments; and in 1913 Niels Bohr, by taking the discrete states of the atoms seriously and extending Planck's assumptions in two directions with great ingenuity, but irrefutable consistency, could explain quantitatively some of the atomic line spectra, which are all patently *discrete*, and which had in their entirety formed a great conundrum up to then : Bohr's theory turned them into the ultimate and irrevocable direct evidence, that the discrete states are a genuine and real fact. Bohr's theory held the ground for about a dozen of years, scoring a grand series of so marvellous and genuine successes, that we may well claim excuses for having shut our eyes to its one great deficiency : while describing minutely the so-called ' stationary ' states which the atom had normally, i.e. in the comparatively uninteresting periods when *nothing happens*, the theory was silent about the periods of transition or ' quantum jumps ' (as one then began to call them). Since intermediary states had to remain disallowed, one could not but regard the transition as instantaneous ; but on the other hand, the radiating of a coherent wave train of 3 or 4 feet length, as it can be observed in an interferometer, would use up just about the average interval between two transitions, leaving the atom no time to ' be ' in those stationary states, the only ones of which the theory gave a description.

This difficulty was overcome by quantum mechanics, more especially by wave mechanics, which furnished a new description of the *states ;* this was precisely what was still missing in the earliest version of the new theory which had preceded wave mechanics by about one year. The previously admitted discontinuity was not abandoned, but it shifted from the *states* to something else, which is most easily grasped by the simile of a vibrating string or drumhead or metal plate, or of a bell that is tolling. If such a body is struck, it is set vibrating, that is to say it is slightly deformed and then runs in rapid succession through a continuous series of slight deformations again and again. There is, of course, an infinite variety of ways of striking a given body, say a bell, by a hard or soft, sharp or blunt instrument, at different points or at several points at a time. This produces an infinite variety of initial deformations and accordingly a truly infinite variety of shapes of the ensuing vibration : the rapid ' succession of cinema pictures,' so we might call it, which describes the vibration following on a particular initial deformation is infinitely manifold. But in every case, however complicated the actual motion is, it can be mathematically analysed as being the *superposition* of a discrete series of comparatively simple ' proper vibrations,' each of which goes on with a quite definite frequency. This discrete series of frequencies depends on the shape and on the material of the body, its density and elastic properties. It can be computed from the theory of elasticity, from which the existence and the discreteness of proper modes and proper frequencies, and the fact that any possible vibration of that body can be analysed into a superposition of them, are very easily deduced quite generally, i.e. for an elastic body of any shape whatsoever.

The achievement of wave mechanics was, that it found a general model picture in which the ' stationary ' states of Bohr's theory take the rôle of proper vibrations, and their discrete ' energy levels ' the rôle of the proper frequencies of these proper vibrations ; and all this follows from the new theory, once it is accepted, as simply and neatly as in the theory of elastic bodies, which we mentioned as a simile. Moreover, the radiated frequencies, observed in the line spectra, are in the new model, equal to the *differences* of the proper frequencies ; and this is easily understood, when two of them are acting simultaneously, on simple assumptions about the nature of the vibrating ' something.'

3 *The Alleged Energy Balance—a Resonance Phenomenon*

But to me the following point has always seemed the most relevant, and it is the one I wish to stress here, because it has been almost obliterated—if words mean something, and if certain words now in general use are taken to mean what they say. The principle of superposition not only bridges the gaps between the ' stationary ' states, and allows, nay compels us, to admit intermediate states without removing the discreteness of the ' energy levels ' (because they have become proper frequencies) ; but it completely *does away with the prerogative of the stationary states.* The epithet stationary has become obsolete. Nobody who would get acquainted with wave mechanics without knowing its predecessor (the Planck-Einstein-Bohr-theory) would be inclined to think that a wave-mechanical system has a predilection for being affected by only one of its proper modes at a time. Yet this is implied by the continued use of the words ' energy levels,' ' transitions,' ' transition probabilities.'

The perseverance in this way of thinking is understandable, because the great and genuine successes of the idea of energy parcels has made it an ingrained habit to regard the product of Planck's constant *h* and a frequency as a bundle of energy, lost by one system and gained by another. How else should one understand the exact dove-tailing in the great ' double-entry ' book-keeping in nature ? I maintain that it can in all cases be understood as a resonance phenomenon. One ought at least to try, and look upon atomic frequencies just as frequencies and drop the idea of energy-parcels. I submit that the word ‘ energy ’ is at present used with two entirely different meanings, macroscopic and microscopic. Macroscopic energy is a ‘ quantity-concept ’ (Quantitätsgrösse). Microscopic energy meaning $h\nu$) is a ‘ quality-concept ’ or ‘ intensity-concept ’ (Intensitätsgrösse) ; it is quite proper to speak of high-grade and low-grade energy according to the value of the frequency ν. True, the macroscopic energy is, strangely enough, obtained by a certain weighted summation over the frequencies, and in this relation the constant h is operative. But this does not necessarily entail that in every single case of microscopic interaction a whole portion $h\nu$ of *macroscopic* energy is exchanged. I believe one is allowed to regard microscopic interaction as a continuous phenomenon without losing either the precious results of Planck and Einstein on the equilibrium of (macroscopic) energy between radiation and matter, or any other understanding of phenomena that the parcel-theory affords.

The one thing which one has to accept and which is the inalienable consequence of the wave-equation as it is used in every problem, under the most various forms, is this : that the interaction between two microscopic physical systems is controlled by a peculiar law of resonance. This law requires that the *difference* of two proper frequencies of the one system be equal to the difference of two proper frequencies of the other :

$$\nu_1 - \nu'_1 = \nu_2' - \nu_2. \qquad (1)$$

The interaction is appropriately described as a gradual change of the amplitudes of the four proper vibrations in question. People have kept to the habit of multiplying this equation by h and saying it means, that the first system (index 1) has dropped from the energy level $h\nu_1$ to the level $h\nu_1'$, the balance being transferred to the second system, enabling it to rise from $h\nu_2$ to $h\nu_2'$. This interpretation is obsolete. There is nothing to recommend it, and it bars the understanding of what is actually going on. It obstinately refuses to take stock of the principle of superposition, which enables us to envisage simultaneous gradual changes of any and all amplitudes without surrendering the essential discontinuity, if any, namely that of the frequencies. To be accurate we must add, that the condition of resonance, equation (1), may include three or more interacting systems. It may for example read

$$\nu_1 - \nu_1' = \nu_2' - \nu_2 + \nu_3' - \nu_3. \qquad (2)$$

Moreover we may adopt the view that the two or more interacting systems are regarded as *one* system. One is then inclined to write equations (1) and (2), respectively, as follows

$$\nu_1 + \nu_2 = \nu_1' + \nu_2' \quad . \quad . \quad . \quad . \quad . \quad . \qquad (1')$$

$$\nu_1 + \nu_2 + \nu_3 = \nu_1' + \nu_2' + \nu_3' \quad , . \quad . \quad . \quad . \qquad (2')$$

and to state the resonance condition thus : the interaction is restricted to constituent vibrations of the *same* frequency. This is a familiar state of affairs, of old. Unfamiliar is the tacit admission that frequencies are *additive,* when two or more systems are considered as forming *one* system. It is an inevitable consequence of wave mechanics. Is it so very repugnant to common sense ? If I smoke 25 cigarettes per day, and my wife smokes 10, and my daughter 12—is not the family consumption 47 per day—on the average ?

4 *A Typical Experiment*

Jokes aside, I wish to consider some typical experiments that ostensibly force the energy parcel view upon us, and I wish to show that this is an illusion. A beam of cathode rays of uniform velocity, which can be gradually increased, is passed through sodium vapour. Behind the vessel containing the vapour the beam passes an electric field which deflects it and tells us the velocity of the particles after the passage. At the same time a spectrometer inspects the light, if any, emitted by the vapour. For small initial velocity nothing happens : no light, no change of velocity in the cathode beam. But when the initial velocity is increased beyond a sharply defined limit, two things happen. The vapour begins to glow, radiating the frequency of the first line of the ' principal series ' ; and the beam of cathode rays emerging from the vapour is split into two by the deflecting electric field, one indicating the initial velocity unchanged, and another slow one has ' lost an amount of energy ' equal to the frequency of the said spectral line multiplied by Planck's constant *h.* If the velocity is further increased the story repeats itself when the incident cathode ray energy increases beyond the ' energy level ' that is responsible for the second line (or rather the ' level-difference ' in question) ; this line appears and a third beam of cathode rays with correspondingly reduced speed occurs ; and so on. This was, and still is, regarded as blatant evidence of the energy parcel view.

But it is just as easily understood from the resonance point of view. A cathode ray of particles with uniform velocity is a monochromatic beam of de Broglie waves. Only when its frequency (ν_1) surpasses the frequency difference ($\nu_2' - \nu_2$) between the lowest (ν_2) and the second (ν_2') proper frequencies of the sodium atom is there a de Broglie frequency $\nu_1' > 0$ that fulfils the resonance demand, equation (1). Then the vibration ν_1' appears in the de Broglie wave and ν_2' among the atoms which begin to glow with frequency $\nu_2' - \nu_2$, since Maxwell's 'electromagnetic vacuum' is prepared for resonance with anything. The splitting of the cathode ray beam in the deviating electric field, after passing the vapour, is accounted for by de Broglie's wave equation. An electric field has for de Broglie waves an 'index of refraction' that *depends* on their frequency ('dispersion') and has a gradient in the direction of the field (which thus acts as an 'inhomogeneous medium'). Any further events that might happen, for instance a transfer of some of the 'energy quanta' $h(\nu_2' - \nu_2)$ from the sodium atoms to other gas molecules by 'impacts of the second kind,' are just as easily understood as resonance phenomena, provided only one keeps to the wave picture throughout and for all particles involved.

Many similar cases of apparent transfer of energy-parcels can be reduced to resonance—for instance photochemical action. The pattern is always the same: you may either take equations like (1) or (2) as they stand (resonance), or multiply them by h and think they express an energy balance of every single micro-transition. In the preceding example one point is of particular interest. One is able by an external agent (the electric field) to *separate in space* the two or more frequencies which have arisen in the cathode ray by the interaction; for they behave differently towards this agent and the different behaviour is completely understood from de Broglie's wave equation; one thus obtains two or more beams of homogeneous frequency (or velocity). It is extremely valuable that there are simple cases of this kind in which the separation into two 'phases' has nothing enigmatic; it is an immediate consequence of the principles laid down in L. de Broglie's earliest work on material waves. I say, this is fortunate; for there is a vast domain of phenomena in which the separation in space either takes place in the natural conditions of observation, or can easily be brought about by simple appliances; but it is not as easily explained on first principles. This might dishearten one in accepting the view of gradually changing amplitudes, that I put forward here; for the separation into different phases that produces itself before our eyes seems to confirm the belief that a discontinuous abrupt and *complete* transition occurs in every single microscopic interaction.

5 *Chemistry, Photochemistry, and the Photoelectric Effect*

The vast amount of phenomena I am alluding to is in the first place ordinary chemistry. Two or more constituents, mixed in a solution or in a gaseous phase, begin to react with each other, under the influence of light or otherwise ; the portions that have reacted and have formed a new chemical compound may separate themselves almost entirely from the rest and form a new phase, say because the product is almost insoluble in the liquid, or (in the case of a gaseous mixture) by its being a liquid or solid with a low vapour pressure at the temperature in question. Almost any chemical reaction may serve as an example, but let us take a slow one to facilitate speech and thought. If a suitable mixture of hydrogen gas (H_2) and oxygen gas (O_2) is illuminated by ultraviolet light, the following slow reaction is induced

$$2H_2 + O_2 \rightarrow 2H_2O \quad . \quad . \quad . \quad . \qquad (3)$$

As the concentration of water vapour (H_2O) increases, part of it separates off into liquid droplets.

The actual process is not as simple as the balance (3) indicates, it is a chain reaction. But we need pay no attention to this, and contemplate only the initial state and the end-product. Wave-mechanically the gaseous mixture is represented by a vibration of the combined system, and, by the way, not by *one* proper vibration since there is anyhow the vast variety of translational and rotational modes, and, of course, the electronic modes. The gaseous compound, H_2O, is represented by an entirely different vibration of the *same* system. The modes composing it, absent at first, are gradually chiming in as the reaction proceeds. But then there is a *third* group of vibrations representing the liquid H_2O ; they gradually build up where they are facilitated by dust nuclei, and are observed as droplets. It is, of course, deplorable that wave mechanics does not allow us to follow this *observed* process analytically, while, in the now current interpretation, ample information is forthcoming about a host of experiments that nobody has ever been or ever will be able to perform (for instance we are told, what is the probability of our finding at a definite spot inside a given hydrogen atom an electron, if we look for one). But there is no reason to suspect that the separation of phases is fundamentally different from the spectroscopic resolution of a beam of light or of cathode rays into its monochromatic constituents. One need not be afraid that the formation of spatial boundaries, separating coherent regions of chemically or physically distinct properties, cannot possibly be controlled by the wave equation, but must necessarily be accounted for by the picturesque pageantry of individual molecules swallowing or re-spewing whole energy parcels, being disrupted and re-combined, until they eventually go to form one or two molecules of a new type.

I deem the latter simply wrong ; it is not in accordance with our present state of knowledge, whose further progress is hampered if these easy pictures, that are in common use, are taken literally. And we are encouraged to take them literally not only by text-books and popular essays but also by the language used in very high-browed technical treatises. By this I will not deny that this imagery is a very useful, nay indispensable, conceptual shorthand in chemical research. One cannot see how to avoid it when, for example, a complicated chain reaction is to be unravelled. And, of course, the chemical equation for describing a reaction will never be ousted, though it ostensibly describes the single micro-event and is wrong in this. It is an instance of the famous 'as if.' It is not the first instance of this kind in the relation of chemistry and physics. The chemist used the valency stroke for building models of complicated molecules. It represented very real facts of observation. For a long time the physicist could not afford any explanation of the mechanism of the chemical bond. Then, in brief succession, *two* were given : there is a heteropolar bond (Kossel, 1916) and a homopolar bond (London-Heitler, 1926). The discoveries were illuminating to the chemist, indeed they removed some difficulties caused by interpreting the valency strokes too naïvely. But, of course, the valency strokes were retained as an extremely convenient shorthand. They could be retained because they were based on carefully pondered observation.

As one of the simplest photochemical reactions we may regard the photoelectric effect, which was one of the main incentives for Einstein in 1905 to launch the hypothesis of light quanta. When a metal plate is illuminated by light of sufficiently high *frequency*, electrons emerge from it forthwith with an energy corresponding to this frequency. There is no time delay, even when the *intensity* of the incident light is so weak that according to the electron theory of H. A. Lorentz, which was at the time in full swing, an electron would need half an hour to be sped up to the velocity in question. This was—and, I am afraid, still is—regarded as convincing evidence of the instantaneous transfer of whole quanta of energy from the light to the electron. I understand the present orthodox interpretation to be as follows. The incident light beam produces at once in each of tens of thousands of electrons an exceedingly small *probability* of taking within the next split second a leap into a state of higher translational energy ; a correspondingly small fraction of those tens of thousands do so and emerge from the metal, and that is why the game starts without delay.

But according to wave mechanics, as put forward by de Broglie and myself and generally accepted, the interpretation does produce without delay electronic wave trains of the higher frequency that we observe emerging from the metal. (For to observe the *frequency* of an electron or its *velocity* means the same thing.) After this has been recognised, is the probability scheme any longer needed ? Has the idea of the mysterious sudden leaps of single electrons not become gratituitous ? Is it expedient ? The waves are there anyhow, and we are not at a loss to prove it. We need only put a tube of crystal powder in the way of the emerging beam and produce an interference pattern of the type first achieved by G. P. Thomson (it might not be as beautiful as Thomson's, but it would vouch for the waves all the same).

6 *Single Interaction Processes between Micro-Systems* (' *Collisions* ')

There are besides chemistry several other domains of theoretical investigation in which the simplifying scheme of individual constituent micro-systems on sharp energy levels, with abrupt transitions between them, affords a very convenient shorthand. Nearly all thermodynamical considerations are greatly facilitated by adopting this scheme in speech and thought, which makes very little difference, if any, in the results. This constitutes a certain danger. In the inseparable union of speech and thought the primacy, rather paradoxically, rests with speech. When we hear the same words again and again pronounced with authority, we are apt to forget that they were originally meant as an abbreviation ; we are induced to believe that they describe a reality.

If the simplified scheme of sharp energy states and abrupt transitions between them was workable throughout in all instances (which

I do not think is the case), one would have to try and cast it into a consistent theory. At the moment no such theory exists and I can see no prospect of obtaining one, nor any inducement to try, for the reason just mentioned in parenthesis. At present the scheme is inconsistent, not only because of the mystery the *transitions* continue to present from their first appearance in the theories of Planck (1900) and Bohr (1913), but also for another reason, intimately connected with the former. In application to two individual micro-systems which interact, it is not at all clear which are the pure energy levels distinguished, to which the scheme shall apply. The choice rests with the mathematical technique. The usual procedure is as follows. The total energy (which enters the mathematical analysis as the ' operator ' of the wave equation) is regarded as made up of three additive parts ; the two main contributions are said to pertain to the single systems and are said to control their behaviours, respectively, if they did *not* interact ; the third is said to be their energy (or ' operator ') of interaction. But this partition is rather artificial, at any rate whilst the interaction takes place. It is largely guided by the requirement that the main parts should be comparatively simple and easy to deal with analytically, the whole complication of the problem being shoved into the interaction, which is called a perturbation and dealt with by methods of approximation. Even so, it is hardly ever amenable to a true solution (albeit an approximate one) ; one has to content oneself with finding out what happens in a small interval of time. One computes the very small changes of amplitudes that occur during this short interval ; and one is pleased to call the time-rate of change the *probability of transition*. By calling it so one expresses the belief that after the interaction has taken place and the two systems have separated again each of them will find itself in a pure sharp energy state. The computation does *not* give this result. The computation tells us, that in either system a host of pure energy-states will be superposed—with a certain dependence between the partial amplitudes in the one system and those in the other. But one chooses to interpret this result as meaning that there will be *complete* exchange between only *one* pair of proper modes, one of the many for which the resonance condition holds.

One might say, why not, if this interpretation works and if it is consistent ? I maintain that it is inconsistent. The reason is the following. Assume each of the two systems found itself in a pure energy state, when they were isolated, before the interaction started.

Now let the interaction set in ; take it to be very weak. It is certainly legitimate to adopt the attitude that we are now faced with *one* system ; nay, this is the fundamentally correct attitude to take ; the splitting of the wave-operator (' energy ') into two private parts plus a part depicting the interaction is only a mathematical artifice. But however weak the interaction be, it has the *immediate* consequence that the combined system is now very far from any one of *its* pure energy states. This is not the result of a very strong mutual physical influence. It obtains prior to any physical change. It results from a slight distuning of the proper modes by the perturbation. What were clear ' one tone ' proper modes in the isolated systems no longer are in the combined ' dis-tuned ' systems—*not nearly*. You have to re-shuffle them and combine (superpose them) *in your mind* in an intriguing fashion to find the proper modes of the combined system. I say ' in your mind '—there is, of course, no immediate physical re-shuffling ; you just state that your combined system is very far from finding itself in one of its proper modes. And that is the very reason why, as time goes on, anything will happen at all, and why, in fact, even a weak interaction, given time, will produce substantial changes of the amplitudes. For it is a simple elementary and universally recognised statement of wave mechanics that an isolated system that vibrates exactly in one of its proper modes undergoes no change whatsoever.

This has, by the way, a consequence worth mentioning. When we spoke just before of a weak interaction setting in between the previously isolated systems, the reader may very naturally have pictured to himself the two systems being first at great distance and then approaching each other and getting in contact. I avoided this description on purpose, because it would flatly contradict the assumption that the isolated systems were in pure energy states. *If so they cannot be said to approach each other.* To think of atoms and molecules *in pure energy states*, moving hither and thither, colliding and rebounding contradicts the fundamental concepts of the theory. Where anything *happens*, we are not facing pure energy states. So obviously we never are.

Let me return for a moment to our two micro-systems in weak interaction or, as I prefer to say, to a system consisting of two parts in slight coupling. The state of affairs is simply this : if this system as a whole settled down in one of its exact proper modes, this would *not* be a state which *the current view* interprets as indicating a definite partition of the total energy between the two parts—*not nearly ;*

I mean to say, it is not a question of slight fluctuation or uncertainty, but of many widely different partitions. If you abolished the coupling at this moment, the now isolated parts would vibrate, each of them, in a superposition of *widely* different proper modes.

Summarising: the current view, which privileges the 'sharp energy states,' is self-contradictory, anyhow in the language it uses (what people mean, when they say something else than they mean, is difficult to guess). We found it self-contradictory in that it cannot be maintained for both the whole and the parts; we are left to choose and to apply the privilege the way it is most convenient. We found a minor inconsistency in the apparently innocent statement that two systems (both of sharply defined energy) *approach* each other and collide. This seems a little less obnoxious, because it can be evaded by saying: Oh well, we do not mean really quite sharp. Some may consider this point a rather gratuitous nagging. I wonder whether in actual collision problems it is entirely irrelevant.

(*The concluding part of this article will be published in November.*)

NOTE

Professor Michael Polanyi drew my attention to a mistake. My description of the Franck-Hertz experiment in the first paragraph of Section 4 is, to put it mildly, oversimplified. The cathode ray beam is appreciably scattered in the vapour. The two or more electronic frequencies that emerge could therefore hardly be separated by the simple transversal field method. But any two of them can be separated by a potential barrier which the one can penetrate, while the other is turned back, being totally reflected. Since this is also completely understood by de Broglie's wave equation, the main argument is not impaired.

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THE NATURE OF PHILOSOPHICAL PROBLEMS AND THEIR ROOTS IN SCIENCE *

K. R. Popper

I

It was after some hesitation that I decided to take as my point of departure the present position of English philosophy. For I believe that the function of a scientist or of a philosopher is to solve scientific or philosophical problems, rather than to talk about what he or other philosophers are doing or might do. Even an unsuccessful attempt to solve a scientific or philosophical problem, if it is an honest and devoted attempt, appears to me more significant than any discussion of a question such as 'What is science ?' or 'What is philosophy ?'. And even if we put this latter question, as we should, in the somewhat improved form 'What is the character of philosophical problems ?', I for one should not bother much about it ; I should feel that it has little weight if compared with even such a minor problem of philosophy, as, say, the question whether every discussion must always proceed from 'assumptions' or 'suppositions' which themselves are beyond argument.[1]

When describing 'What is the character of philosophical problems ?' as a somewhat improved form of 'What is philosophy ?', I wished to hint at one of the reasons for the futility of the current controversy concerning the nature of philosophy—the naive belief that there is an entity such as 'philosophy', or perhaps 'philosophical activity', and that it has a certain character or 'nature'. The belief that there is such a thing as physics, or biology, or archaeology, and that these 'studies' or 'disciplines' are distinguishable by the subject matter which they investigate, appears to me to be a residue from the time when one believed that a theory had to proceed from a definition of its own subject matter.[2] But subject matter, or kinds of things, or classes

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* The Chairman's address, delivered at the meeting of 28 April 1952, to the Philosophy of Science Group of the British Society for the History of Science.

[1] I call this a minor problem because I believe that it can easily be solved, by refuting the ('relativistic') doctrine indicated in the text.

[2] This view is part of what I have called 'essentialism'. Cf. for example my *Open Society*, ch. 11, or 'The Poverty of Historicism I' (*Economica* N.S., 1944, **11**, No. 42).

of things, do not, I hold, constitute a basis for the distinction of disciplines. Disciplines are distinguished partly for historical reasons and reasons of administrative convenience (such as the organisation of teaching and of appointments), partly because the theories which we construct to solve our problems have a tendency[1] to grow into unified systems. But all this classification and distinction is a comparatively unimportant and superficial affair. *We are not students of subject matter but students of problems.* And problems may cut right across the borders of any subject matter or discipline.

Obvious as this fact may appear to some people, it is so important for our present discussion that it is worth while to illustrate it by an example. It hardly needs mentioning that a problem posed to a geologist such as the assessment of the chances of finding deposits of oil or of uranium in a certain district needs for its solution the help of theories and techniques usually classified as mathematical, physical, and chemical. It is, however, less obvious that even a more ' basic ' science such as atomic physics may have to make use of a geological survey, and of geological theories and techniques, if it wishes to solve a problem arising in one of its most abstract and fundamental theories ; for example, the problem of testing predictions concerning the relative stability or instability of atoms of an even or odd atomic number.

I am quite ready to admit that many problems, even if their solution involves the most diverse disciplines, nevertheless ' belong ', in some sense, to one or another of the traditional disciplines ; for example, the two problems mentioned ' belong ' clearly to geology and physics respectively. This is due to the fact that each of them arises out of a discussion which is characteristic of the tradition of the discipline in question. It arises out of the discussion of some theory, or out of empirical tests bearing upon a theory ; and theories, as opposed to subject matter, may constitute a discipline (which might be described as a somewhat loose cluster of theories undergoing a process of challenge, change, and growth). But this does not alter the view that the classification into disciplines is comparatively unimportant, and that we are students, not of disciplines, but of problems.

But are there philosophical problems ? The present position of English philosophy, which I shall take as my point of departure, originates, I believe, from the late Professor Ludwig Wittgenstein's influential doctrine that there are none; that all genuine problems are scientific problems; that the alleged problems of philosophy are pseudo-problems; that the alleged propositions or theories of philosophy are pseudo-propositions or pseudo-theories; that they are not false (if false,[1] their negations would be true propositions or theories) but strictly meaningless combinations of words, no more meaningful than the incoherent babbling of a child who has not yet learned to speak properly.[2]

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[1] This tendency can be explained by the principle that theoretical explanations are the more satisfactory the better they can be supported by *independent* evidence. (This somewhat cryptic remark cannot, I fear, be amplified in the present context.)

As a consequence, philosophy cannot contain any theories. Its true nature, according to Wittgenstein, is not that of a theory, but that of an activity. The task of all genuine philosophy is that of unmasking philosophical nonsense, and of teaching people to talk sense.

My plan is to take this doctrine [3] of Wittgenstein's as my starting point (section 2). I shall try (in section 3) to explain it ; to defend it, to some extent ; and to criticise it. And I shall support all this (in sections 4 to 6) by some examples from the history of scientific ideas.

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[1] It is of particular importance in this connection to realise that Wittgenstein's use of the term 'meaningless' is *not* the usual and somewhat vague one according to which an absurdly false assertion (such as '$2 + 3 = 5{\cdot}427$' or 'I can play Bach on the adding machine') may be called 'meaningless'. He called a statement-like expression 'meaningless' only if it is not a properly constructed statement at all, and therefore neither true nor false. Wittgenstein himself gave the example: 'Socrates is identical'.

[2] Since Wittgenstein described his own *Tractatus* as meaningless (see also the next footnote), he distinguished, at least by implication, between revealing and unimportant nonsense. But this does not affect his main doctrine which I am discussing, the non-existence of philosophical problems. (A discussion of other doctrines of Wittgenstein's can be found in the Notes to my *Open Society*, esp. notes 26, 46, 51, and 52 to ch. 11.)

[3] It is easy to detect at once one flaw in this doctrine: the doctrine, it may be said, is itself a philosophic theory, claiming to be true, and not to be meaningless. This criticism, however, is a little too cheap. It might be countered in at least two ways. (1) One might say that the doctrine is indeed meaningless *qua* doctrine, but not *qua* activity. (This is the view of Wittgenstein, who said at the end of his *Tractatus Logico-Philosophicus* that whoever understood the book must realise at the end that it was itself meaningless, and must discard it like a ladder, after having used it to reach the desired height.) (2) One might say that the doctrine is not a philosophical but an empirical one, that it states the historical fact that all 'theories' proposed by philosophers are in fact ungrammatical; that they do not, in fact, conform to the rules inherent in those languages in which they appear to be formulated, that this defect turns out to be impossible to remedy; and that every attempt to express them properly has lead to the loss of their philosophic character (and revealed them, for example, as empirical truisms, or as false statements). These two counter arguments rescue, I believe, the threatened consistency of the doctrine, which in this way indeed becomes 'unassailable', as Wittgenstein puts it by the kind of criticism referred to in this note. (See also the next note but one.)

But before proceeding to carry out this plan, I wish to reaffirm my conviction that a philosopher should philosophise, that is, try to solve philosophic problems, rather than talk about philosophy. If Wittgenstein's doctrine is true, then nobody can, in this sense, philosophise. If this were my opinion, I would give up philosophy. But it so happens that I am not only deeply interested in certain philosophical problems (I do not much care whether they are ' rightly ' called ' philosophical problems'), but possessed by the belief that I may even contribute—if only a little, and only by hard work—to their solution. And my only excuse for talking here about philosophy—instead of philosophising—is, in the last resort, my hope that, in carrying out my programme for this address, an opportunity will offer itself of doing a little philosophising, after all.

2

Ever since the rise of Hegelianism there has existed a dangerous gulf between science and philosophy. Philosophers were accused—rightly, I believe—of ' philosophising without knowledge of fact ', and their philosophies were described as ' mere fancies, even imbecile fancies '.[1] Although Hegelianism was the leading influence in England and on the Continent, opposition to it, and contempt of its pretentiousness, never died out completely. Its downfall was brought about by a philosopher who, like Leibniz, Kant, and J. S. Mill before him, had a sound knowledge of science, especially mathematics. I am speaking of Bertrand Russell.

Russell is also the author of the classification (closely related to his famous *theory of types*) which is the basis of Wittgenstein's view of philosophy, the classification of the expressions of a language into

(1) *True statements*
(2) *False statements*
(3) *Meaningless expressions*, among which there are statement-like sequences of words, which may be called ' pseudo-statements '.

Russell operated with this distinction in connection with the solution of the logical paradoxes which he discovered. It was essential, for this solution, to distinguish, more especially, between (2) and (3). We might say, in ordinary speech, that a false statement, like ' 3 times 4 equals 173 ' or ' All cats are cows ', is meaningless. Russell, however, reserved this characterisation for expressions such as ' 3 times 4 are cows ' or ' All cats equal 173 ', that is, for expressions which are better not described as false statements (as can easily be seen from the fact that their *prima facie* negations, for example, ' Some cats do not equal 173 ' are no more satisfactory than the original expressions) but as pseudo-statements.

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[1] The two quotations are not the words of a scientific critic, but, ironically enough, Hegel's own characterisation of the philosophy of his friend and forerunner Schelling. Cf. my *Open Society*, note 4 (and text) to ch. 12.

Russell used this distinction mainly for the elimination of the paradoxes (which, he indicated, were meaningless pseudo-statements). Wittgenstein went further. Led, perhaps, by the feeling that what philosophers, especially Hegelian philosophers, were saying was somewhat similar to the paradoxes of logic, he used Russell's distinction in order to denounce all philosophy as meaningless.

As a consequence, there could be no genuine philosophical problems. All alleged philosophical problems could be classified into four classes:[1] (1) those which are purely logical or mathematical, to be answered by logical or mathematical propositions, and therefore not philosophical; (2) those which are factual, to be answered by some statement of the empirical sciences, and therefore again not philosophical; (3) those which are combinations of (1) and (2), and therefore, again, not philosophical; and (4) meaningless pseudo-problems such as ' Do all cows equal 173 ? ' or ' Is Socrates identical ? ' or ' Does an invisible, untouchable, and apparently altogether unknowable Socrates exist ? '

Wittgenstein's idea of eradicating philosophy (and theology) with the help of an adaption of Russell's theory of types was ingenious and original (and more radical even than Comte's positivism which it resembles closely).[2] This idea became the inspiration of the powerful modern school of language analysts who have inherited his belief that there are no genuine philosophical problems, and that all a philosopher can do is to unmask and dissolve the linguistic puzzles which have been proposed by traditional philosophy.

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[1] Wittgenstein still upheld the doctrine of the non-existence of philosophical problems in the form here described when I saw him last (in 1946, when he presided over a stormy meeting of the Moral Science Club in Cambridge, on the occasion of my reading a paper on ' Are there Philosophical Problems ? '). Since I had never seen any of his unpublished manuscripts which were privately circulated by some of his pupils, I had been wondering whether he had modified what I here call his ' doctrine ' ; but I found his views on this most fundamental and influential point of his teaching unchanged.

[2] Cf. note 52 (2) to ch. 11 of my *Open Society*.

My own view of the matter is that only as long as I have genuine philosophical problems to solve shall I continue to take an interest in philosophy. I fail to understand the attraction of a philosophy without problems. I know, of course, that many people talk nonsense ; and it is conceivable that it should become one's task (an unpleasant one) to unmask somebody's nonsense, for it may be dangerous nonsense. But I believe that some people have said things which were not very good sense, and certainly not very good grammar, but which are at the same time highly interesting and exciting, and perhaps more worth listening to than the good sense of others. I may mention the differential and integral calculus which, especially in its early forms, was, no doubt, completely paradoxical and nonsensical by Wittgenstein's (and other) standards ; which became, however, reasonably well founded as the result of some hundred years of great mathematical efforts ; but whose foundations even at this very moment are still in need, and in the process, of clarification.[1] We might remember, in this context, that it was the contrast between the apparent absolute precision of mathematics and the vagueness and inprecision of philosophical language which deeply impressed the earlier followers of Wittgenstein. But had there been a Wittgenstein to use his weapons against the pioneers of the calculus, and had he succeeded in the eradication of their nonsense, where their contemporary critics (such as Berkeley who was, fundamentally, right) failed, then he would have strangled one of the most fascinating and philosophically important developments in the history of thought. Wittgenstein once wrote : 'Whereof one cannot speak, thereof one must be silent.' It was, if I remember rightly, Erwin Schroedinger who replied : 'But it is only here that speaking becomes interesting.' The history of the calculus—and perhaps of his own theory [2]—bears him out.

No doubt, we should all train ourselves to speak as clearly, as precisely, as simply, and as directly as we can. But I believe that there is not a classic of science, or of mathematics, or indeed a book worth reading that could not be shown, by a skilful application of the technique of language analysis, to be full of meaningless pseudo-propositions and what some people might call ' tautologies '.

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[1] I am alluding to G. Kreisel's recent construction of a monotone bounded sequence of rationals every term of which can be actually computed, but which does not possess a computable limit—in contradiction to what appears to be the *prima facie* interpretation of the classical theorem of Bolzano and Weierstrass, but in agreement with Brouwer's doubts about this theorem. Cf. *Journal of Symbolic Logic*, 1952, **17**, 57.

[2] Before Max Born proposed his famous probability interpretation, Schroedinger's wave equation was, some might contend, meaningless.

3

But I have promised to say something in defence of Wittgenstein's views. What I wish to say is, first, that there is much philosophical writing (especially in the Hegelian school) which may justly be criticised as meaningless verbiage ; secondly, that this kind of irresponsible writing was checked, for a time at least, by the influence of Wittgenstein and the language analysts (although it is likely that the most wholesome influence in this respect was the example of Russell who, by the incomparable charm and the clarity of his writings, established the fact that subtlety of content was compatible with lucidity and unpretentiousness of style).

But I am prepared to admit more. In partial defence of Wittgenstein's view, I am prepared to defend the following two theses.

My first thesis is that every philosophy, and especially every philosophical ' school ', is liable to degenerate in such a way that its problems become practically indistinguishable from pseudo-problems, and its cant, accordingly, practically indistinguishable from meaningless babble. This, I shall try to show, is a consequence of philosophical inbreeding. The degeneration of philosophical schools is the consequence of the mistaken belief that one can philosophise without being compelled to turn to philosophy *by problems which arise outside philosophy*—in mathematics, for example, or in cosmology, or in politics, or in religion, or in social life. To put it in other words, my first thesis is this. *Genuine philosophical problems are always rooted in urgent problems outside philosophy, and they die if these roots decay.* In their efforts to solve them, philosophers are liable to pursue what looks like a philosophical method or like a technique or like an unfailing key to philosophical success.[1] But no such methods or techniques exist ; philosophical methods are unimportant, and any method is legitimate if it leads to results capable of being rationally discussed. What matters is neither methods nor techniques—nothing but a sensitiveness to problems, and a consuming passion for them ; or as the Greeks said, the gift of wonder.

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[1] It is very interesting that the imitators were always inclined to believe that the ' master ' did his work with the help of a secret method or a trick. It is reported that in J. S. Bach's days some musicians believed that he possessed a secret formula for the construction of fugue themes.

There are those who feel the urge to solve a problem, those for whom the problem becomes real, like a disorder which they have to get out of their system.[1] They will make a contribution even if they use a method or a technique. But there are others who do not feel this urge, who have no serious and pressing problem but who nevertheless produce exercises in fashionable methods, and for whom philosophy is *application* (of whatever insight or technique you like) rather than *search.* They are luring philosophy into the bog of pseudo-problems and verbal puzzles ; either by offering us pseudo-problems for real ones (the danger which Wittgenstein saw), or by persuading us to concentrate upon the endless and pointless task of unmasking what they rightly or wrongly take for pseudo-problems (the trap into which Wittgenstein fell).

My second thesis is that what appears to be the *prima facie* method of teaching philosophy is liable to produce a philosophy which answers Wittgenstein's description. What I mean by '*prima facie* method of teaching philosophy ', and what would seem to be the only method, is that of giving the beginner (whom we take to be unaware of the history of mathematical, cosmological, and other ideas of science as well as of politics) the works of the great philosophers to read ; say, of Plato and Aristotle, Descartes and Leibniz, Locke, Berkeley, Hume, Kant, and Mill. What is the effect of such a course of reading ? A new world of astonishingly subtle and vast *abstractions* opens itself to the reader, abstractions of an extremely high and difficult level. Thoughts and arguments are put before his mind which sometimes are not only hard to understand, but whose relevance remains obscure since he cannot find out what they may be relevant to. Yet the student knows that these are the great philosophers, that this is the way of philosophy. Thus he will make an effort to adjust his mind to what he believes (mistakenly, as we shall see) to be their way of thinking. He will attempt to speak their queer language, to match the torturous spirals of their argumentation, and perhaps even tie himself up in their curious knots. Some may learn these tricks in a superficial way, others may begin to become genuinely fascinated addicts. Yet I feel that we ought to respect the man who, having made his effort, comes ultimately to what may be described as Wittgenstein's conclusion : 'I have learned the jargon as well as anybody. It is very clever and captivating. In fact, it is dangerously captivating ; for the simple truth about the matter is that it is much ado about nothing —just a lot of nonsense.'

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[1] I am alluding to a remark by Professor Gilbert Ryle, who says on page 9 of his *Concept of Mind* : 'Primarily I am trying to get some disorders out of my own system.'

Now I believe such a conclusion to be grossly mistaken ; it is, however, the almost inescapable result, I contend, of the *prima facie* method of teaching philosophy here described. (I do not deny, of course, that some particularly gifted students may find very much more in the works of the great philosophers than this story indicates— and without deceiving themselves.) For the chance of finding out the extra-philosophical problems (the mathematical, scientific, moral and political problems) which inspired these great philosophers is very small indeed. These problems can be discovered, as a rule, only by studying the history of, for example, scientific ideas, and especially the problem-situation in mathematics and the sciences of the period in question ; and this, in turn, presupposes a considerable acquaintance with mathematics and science. Only an understanding of the contemporary problem-situation in the sciences can enable the student of the great philosophers to understand that they tried to solve urgent and concrete problems ; problems which, they found, could not be dismissed. And only after understanding this fact can a student attain a different picture of the great philosophies—one which makes full sense of the apparent nonsense.

I shall try to establish my two theses with the help of examples ; but before turning to these examples, I wish to summarise my theses, and to balance my account with Wittgenstein.

My two theses amount to the contention that philosophy is deeply rooted in non-philosophical problems ; that Wittengstein's negative judgment is correct, by and large, as far as philosophies are concerned which have forgotten their extra-philosophical roots ; and that these roots are easily forgotten by philosophers who 'study' philosophy, instead of being forced into philosophy by the pressure of non-philosophical problems.

My view of Wittgenstein's doctrine may be summed up as follows. It is true, by and large, that pure philosophical problems do not exist ; for indeed, the purer a philosophical problem becomes, the more is lost of its original sense, significance, or meaning, and the more liable is its discussion to degenerate into empty verbalism. On the other hand, there exist not only genuine scientific problems, but genuine philosophical problems. Even if, upon analysis, these problems turn out to have factual components, they need not be classified as belonging to science. And even if they should be soluble by, say, purely logical means, they need not be classified as purely logical or tautological. Analogous situations arise in physics. For example, the explanation or prediction of certain spectral terms (with the help of a hypothesis concerning the structure of atoms) may turn out to be soluble by purely mathematical calculations. But this, again, does not imply that the problem belonged to pure mathematics rather than to physics. We are perfectly justified in calling a problem 'physical' if it is connected with problems and theories which have been traditionally discussed by physicists (such as the problems of the constitution of matter), even if the means used for its solution turn out to be purely mathematical. As we have seen, the solution of problems may cut through the boundary of many sciences. Similarly, a problem may be rightly called 'philosophical' if we find that, although originally it may have arisen in connection with, say, atomic theory, it is more closely connected with the problems and theories which have been discussed by philosophers than with theories nowadays treated by physicists. And again, it does not matter in the least what kind of methods we use in solving such a problem. Cosmology, for example, will always be of great philosophical interest, even though by some of its methods it has become closely allied to what is perhaps better called 'physics'. To say that, since it deals with factual issues, it must belong to science rather than to philosophy, is not only pedantic but clearly the result of an epistemological, and thus of a philosophical, dogma. Similarly, there is no reason why a problem soluble by logical means should be denied the attribute 'philosophical'. It may well be typically philosophical, or physical, or biological. For example, logical analysis played a considerable part in Einstein's special theory of relativity; and it was, partly, this fact which made this theory philosophically interesting, and which gave rise to a wide range of philosophical problems connected with it.

Wittgenstein's doctrine turns out to be the result of the thesis that all genuine statements (and therefore all genuine problems) can be classified into one of two exclusive classes: factual statements (*synthetic a posteriori*), and logical statements (*analytic a priori*). This simple dichotomy, although extremely valuable for the purposes of a rough survey, turns out to be for many purposes too simple.[1] But although it is, as it were, specially designed to exclude the existence of philosophical problems, it is very far from achieving even this aim; for even if we accept the dichotomy, we can still claim that factual or logical or mixed problems may turn out, in certain circumstances, to be philosophical.

4

I now turn to my first example: *Plato and the Crisis in Early Greek Atomism.*

My thesis here is that Plato's central philosophical doctrine, the so-called Theory of Forms or Ideas, cannot be properly understood except in an extra-philosophical context[2]; more expecially in the context of the critical problem situation in Greek science[3] (mainly in the theory of matter) which developed as a consequence of *the discovery of the irrationality of the square root of two.* If my thesis is correct, then Plato's theory has not so far been fully understood. (Whether a ' full ' understanding can ever be achieved is, of course, most questionable.) But the more important consequence would be that it can never be understood by philosophers trained in accordance with the *prima facie* method described in the foregoing section—unless, of course, they are specially and *ad hoc* informed of the relevant facts (which they may have to accept on authority).

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[1] Already in my *Logic der Forschung* (Vienna, 1935), I pointed out that a theory such as Newton's may be *interpreted* either as factual or as consisting of implicit definitions (in the sense of Poincaré and Eddington), and that the interpretation which a physicist adopts exhibits itself in his *attitude* towards tests which go against his theory rather than in what he says. The dogma of the simple dichotomy has been recently attacked, on very different lines, by F. H. Heinemann (*Proc. of the Xth Intern. Congress of Philosophy* (Amsterdam, 1949), Fasc. 2, 629, Amsterdam, 1949), by W. van O. Quine, and by Morton G. White. It may be remarked, again from a different point of view, that the dichotomy applies, in a precise sense, only to a formalised language, and therefore is liable to break down for those languages in which we must speak prior to any formalisation, i.e. in those languages in which all the traditional problems were conceived. Some members of the school of the language analysts, however, still believe it a sound method to unmask a theory as 'tautological'.

[2] In my *Open Society and Its Enemies*, I have tried to explain in some detail another extra-philosophical root of the same doctrine, viz. a political root. I also discussed there (in note 9 to ch. 6 of the revised 4th edition, 1952) the problem with which I am concerned in the present section, but from a somewhat different angle. The note referred to and the present section partly overlap; but they are largely supplementary to each other. Relevant references (esp. to Plato) omitted here will be found there.

[3] There are historians who deny that the term 'science' can be properly applied to any development which is older than the sixteenth or even the seventeenth century. But quite apart from the fact that controversies about labels should be avoided, there can, I believe, no longer be a doubt nowadays about the astonishing similarity, not to say identity, of the aims, interests, activities, arguments, and methods, of, say, Galileo and Archimedes, or Copernicus and Plato, or Kepler and Aristarchus (the 'Copernicus of antiquity'). And any doubt concerning the extreme age of scientific

It is well known [1] that Plato's Theory of Forms is historically as well as in its content closely connected with the Pythagorean theory that all things are, in essence, numbers. The details of this connection, and the connection between Atomism and Pythagoreanism, are perhaps not so well known. I shall therefore tell the whole story in brief, as I see it at present.

It appears that the founder of the Pythagorean order or sect was deeply impressed by two discoveries. The first discovery was that a *prima facie* purely qualitative phenomenon such as musical harmony was, in essence, based upon the purely numerical ratios 1 : 2 ; 2 : 3 ; 3 : 4. The second was that the ' right ' or ' straight ' angle (obtainable for example by folding a leaf twice, so that the two folds form a cross) was connected with the purely numerical ratios 3 : 4 : 5, or 5 : 12 : 13 (the sides of rectangular triangles). These two discoveries, it appears, led Pythagoras to the somewhat fantastic generalisation that all things are, in essence, numbers, or ratios of numbers ; or that number was the ratio (*logos* = reason), the rational essence of things, or their real nature.

Fantastic as this idea was, it proved in many ways fruitful. One of its most successful applications was to simple geometrical figures,

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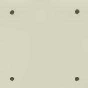
observation, and of careful computations based upon observation, has been dispelled nowadays by the discovery of new evidence concerning the history of ancient astronomy. We can now draw not only a parallel between Tycho and Hipparchus, but even one between Hansen (1857) and Cidenas the Chaldean (314 B.C.), whose computations of the ' constants for the motion of Sun and Moon ' are without exception comparable in precision to those of the best nineteenth-century astronomers, ' Cidenas' value for the motion of the Sun from the Node (0″·5 to great), although inferior to Brown's, is superior to at least one of the most widely used modern values ', wrote J. K. Fotheringham in 1928, in his most admirable article ' The Indebtedness of Greek to Chaldean Astronomy ' (*The Observatory*, 1928, **51**, No. 653), upon which my contention concerning the age of astronomy is based.

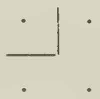
[1] From Aristotle's *Metaphysics*

such as squares, rectangular and isosceles triangles, and also to certain simple solids, such as pyramids. The treatment of some of these geometrical problems was based upon the so-called *gnōmōn.*

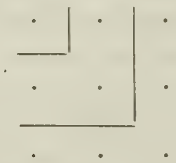
This can be explained as follows. If we indicate a square by four dots,



we may interpret this as the result of adding three dots to the one dot on the upper left corner. These three dots are the first *gnōmōn* ; we may indicate it thus :

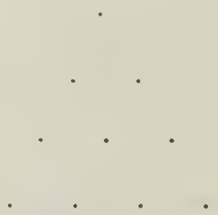


By adding a second *gnōmōn,* consisting of five more dots, we obtain

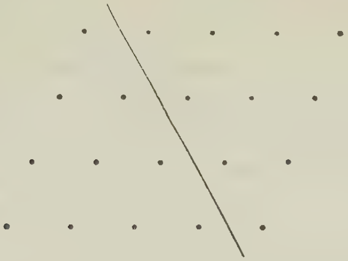


One sees at once that every number of the sequence of the odd numbers, 1, 3, 5, 7 . . . , each forms a *gnōmōn* of a square, and that the sums 1, 1 + 3, 1 + 3 + 5, 1 + 3 + 5 + 7, . . . are the square numbers, and that, if n is the (number of dots in the) side of a square, its area (total number of dots = n^2) will be equal to the sum of the first n odd numbers.

As with the treatment of squares, so with the treatment of isosceles triangles.



Here each *gnōmōn* is a last horizontal line of points, and each element of the sequence 1, 2, 3, 4, . . . is a *gnōmōn.* The ‘ triangular numbers ’ are the sums 1 + 2 ; 1 + 2 + 3 ; 1 + 2 + 3 + 4, etc., that is, the sums of the first n natural numbers. - By putting two such triangles side by side



we obtain the parallelogram with the horizontal side $n + 1$ and the other side n, containing $n(n + 1)$ dots. Since it consists of two isosceles triangles, its number is $2(1 + 2 + \ldots + n)$, so that we obtain the equation

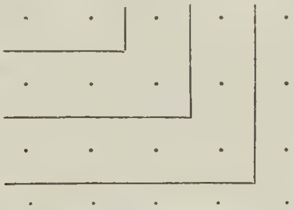
$$1 + 2 + \ldots + n = \tfrac{1}{2}n(n + 1) \tag{3}$$

and

$$d(1 + 2 + \ldots + n) = \frac{d}{2}n(n + 1). \tag{4}$$

From this it is easy to obtain the general formula for the sum of an arithmetical series.

We also obtain 'oblong numbers', that is the numbers of oblong rectangular figures, of which the simplest is



with the oblong numbers $2 + 4 + 6 \ldots$, i.e. the *gnōmōn* of an oblong is an even number, and the oblong numbers are the sums of the even number.

These considerations were extended to solids; for example, by summing the first triangular number, pyramid numbers were obtained. But the main application was to plain figures, or shapes, or 'Forms'. These, it was believed, are characterised by the appropriate sequence of numbers, and thus by the numerical ratios of the consecutive numbers of the sequence. In other words, *'Forms' are numbers or ratios of numbers.* On the other hand, not only shapes of things, but also abstract properties, such as harmony, and 'straightness' are numbers. In this way, the general theory that numbers are the rational essences of all things, is arrived at with some plausibility.

It is very probable that the development of this view was influenced by the similarity of the dot-diagrams with the diagram of a constellation such as the Lion, or the Scorpion, or the Virgo. If a Lion is an arrangement of dots, it must have a number. In this way the belief seems to have arisen that the numbers, or 'Forms', are heavenly shapes of things.

One of the main elements of this early theory was the so-called 'Table of Opposites', based upon the fundamental distinction between odd and even numbers. It contains such things as

ONE	MANY
ODD	EVEN
MALE	FEMALE
REST (BEING)	CHANGE (BECOMING)
DETERMINATE	INDETERMINATE
SQUARE	OBLONG
STRAIGHT	CROOKED
RIGHT	LEFT
LIGHT	DARKNESS
GOOD	BAD

In reading through this strange table one gets some idea of the working of the Pythagorean mind, and why not only the 'Forms' or shapes of geometrical figures were considered to be numbers, in essence, but also abstract ideas, such as Justice and, of course, Harmony, and Health, Beauty and Knowledge. The table is interesting also because it was taken over, with very little alteration, by Plato. Plato's famous theory of 'Forms' or 'Ideas' may indeed be described, somewhat roughly, as the doctrine that the 'Good' side of the Table of Opposites constitutes an (invisible) Universe, a Universe of Higher Reality, of the Unchanging and Determinate 'Forms' of all things, and that True and Certain Knowledge (*epistēmē* = *scientia* = *science*) can be of this Unchanging and Real Universe only, while the visible world of change and flux in which we live and die, the world of generation and destruction, the world of experience, is only a kind of reflection or copy of that Real World. It is only a world of appearance, of which no True and Certain Knowledge can be obtained. What can be obtained in the place of Knowledge (*epistēmē*) are only the plausible but uncertain and prejudiced opinions (*doxa*) of fallible mortals.[1] In his interpretation of the Table of Opposites, Plato was influenced by Parmenides, the man who stimulated the development of Democritus' atomic theory.

Returning now to the original Pythagorean view, there is one thing in it which is of decisive importance for our story. It will have been observed that the Pythagorean emphasis upon Number was fruitful from the point of view of the development of scientific ideas. This is often but somewhat loosely expressed by saying that the Pythagoreans encouraged numerical scientific measurements. Now the point which we must realise is that, for the Pythagoreans, all this was *counting rather than measuring*. It was the counting of numbers, of invisible essences or 'Natures' which were Numbers of little dots or stigmata. Admittedly, we cannot count these little dots directly, since they are invisible. What we actually do is not to count the Numbers or Natural Units, but to measure, i.e. to count arbitrary visible units. But the significance of measurements was interpreted as revealing, indirectly, the true *Ratios of the Natural Units or of the Natural Numbers.*

Thus Euclid's methods of proving the so-called 'Theorem of Pythagoras' (Euclid's I, 47) according to which, if a is the side of a triangle opposite to its right angle between b and c,

$$a^2 = b^2 + c^2, \tag{1}$$

was completely foreign to the spirit of Pythagorean mathematics. In spite of the fact that the theorem was known to the Babylonians and geometrically proved by them, neither Pythagoras nor Plato appear to have known the general *geometrical* proof; for the problem for which they offered solutions, the *arithmetical* one of finding the integral solutions for the sides of rectangular triangles, can be easily solved, if (1) is known by the formula (m and n are natural numbers, and $m > n$)

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[1] Plato's distinction (*epistēmē* vs. *doxa*) derives, I think, from Parmenides (*truth* vs. *seeming*). Plato clearly realised that *all* knowledge of the visible world, the changing world of appearance, consists of *doxa*; that it is tainted by uncertainty even if it utilises the *epistēmē*, the knowledge of the unchanging 'Forms' and of pure mathematics, to the utmost, and even if it interprets the visible world with the help of a theory of the invisible world. Cf. *Cratylus*, 439b ff., *Rep.* 476d ff.; and especially *Timaeus*, 29b ff., where the distinction is applied to those parts of Plato's own theory which we should nowadays call 'physics' or 'cosmology', or, more generally, 'natural science'. They belong, Plato says, to the realm of *doxa* (in spite of the fact that science = *scientia* = *epistēmē*; cf. my remarks on this problem in *The Philosophical Quarterly*, April 1952, p. 168). For a different view concerning Plato's relation to Parmenides, see Sir David Ross, *Plato's Theory of Ideas*, Oxford, 1951, p. 164.

$$(2) \qquad a = m^2 + n^2 ; \; b = 2mn ; \; c = m^2 - n^2.$$

But formula (2) was unknown to Pythagoras and even to Plato. This emerges from the tradition according to which Pythagoras proposed the formula

$$(3) \qquad a = 2m(m + 1) + 1 ; \; b = 2m(m + 1) ; \; c = 2m + 1$$

which can be read off the *gnōmōn* of the square numbers, but which is less general than (2), since it fails, for example, for 8 : 15 : 17. To Plato, who is reported to have improved Pythagoras' formula (3), is attributed another formula which still falls short of the general solution (2).

We now come to the discovery of the *irrationality of the square root of two*. According to tradition, this discovery was made within the Pythagorean order, but was kept secret. (This is suggested by the old term for 'irrational', '*arrhētos*', that is, 'unspeakable', which might well have meant 'the unspeakable mystery'.) This discovery struck at the root of Pythagoreanism; for it meant that such a simple geometrical entity as the diagonal d of the square with the side a could demonstrably not be characterised by *any* ratio of natural numbers; $d : a$ was *no ratio*. The tradition has it that the member of the school who gave away the secret was killed for his treachery. However this may be, there is little doubt that the realisation of the fact that irrational magnitudes (they were, of course, not recognised as numbers) existed, and that their existence could be proved, led to the downfall of the Pythagorean order.

The Pythagorean theory, with its dot-diagrams, contains, no doubt, the suggestion of a very primitive atomism. How far the atomic theory of Democritus was influenced by Pythagoreanism is difficult to assess. Its main influences came, one can say for certain, from the Eleatic School: from Parmenides and from Zeno. The basic problem of this school, and of Democritus, was that of the rational understanding of *change*. (I differ here from the interpretations of Cornford and others.) I think that this problem derives from Ionian rather than from Pythagorean thought, and that it has remained the fundamental problem of Natural Philosophy.

Although Parmenides himself was not a physicist (as opposed to his great Ionian predecessors), he may be described, I believe, as having fathered *theoretical physics*. He produced an anti-physical theory which, however, was the first hypothetical-deductive system. And it was the beginning of a long series of such systems of physical theories each of which was an improvement on its predecessor. As a rule the improvement was found necessary by the realisation that the earlier system was falsified by certain facts of experience. Such an empirical refutation of the consequences of a deductive system leads to efforts at its reconstruction, and thus to a new and improved theory which, as a rule, clearly bears the mark of its ancestry, of the older theory as well as of the refuting experience.

These experiences or observations were, we shall see, very crude at first, but they became more and more subtle as the theories became more and more capable of accounting for the cruder observations. In the case of Parmenides' theory, the clash with observation was so obvious that it would seem perhaps fanciful to describe the theory as the first hypothetical-deductive system of physics. We may, therefore, describe it as the last pre-physical deductive system, whose falsification gave rise to the first truly physical theory, the atomistic theory of Democritus.

Parmenides' theory is simple. He finds it impossible to understand change or movement rationally, and concludes that there is really no change—or that change is only apparent. But before we indulge in feelings of superiority, in the face of such a hopelessly unrealistic theory, we should first realise that there is a serious problem here. If a thing X changes, then clearly it is no longer the same thing X. On the other hand, we cannot say that X changes without implying that X persists during the change ; that it is the same thing X, at the beginning and at the end of the change. Thus, it appears that we arrive at a contradiction, and that the idea of a thing that changes, and therefore the idea of change, is impossible.

All this sounds very philosophical and abstract, and so it is. But it is a fact that the difficulty here indicated has never ceased to make itself felt in the development of physics.[1] And a deterministic system such as that of Einstein's field theory might even be described as a four-dimensional version of Parmenides' unchanging three-dimensional universe. For, in a sense, no change occurs in Einstein's four-dimensional block-universe. Everything is there just as it is, in its four-dimensional *locus* ; change becomes a kind of ' apparent ' change ; it is ' only ' the observer who, as it were, glides along his world-line and becomes successively conscious of the different *loci* along this world-line, that is, of his spatio-temporal surrounding. . . .

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[1] This may be seen from Emile Meyerson's *Identity and Reality*, one of the most interesting philosophical studies of the development of physical theories.

To return from this new Parmenides to the older father of theoretical physics, we may present his deductive theory roughly as follows.

(1) Only what is, is.
(2) What is not does not exist.
(3) Non-being, that is, the void, does not exist.
(4) The world is full.
(5) The world has no parts ; it is one huge block (because it is full).
(6) Motion is impossible (since there is no empty space into which anything could move).

The conclusions (5) and (6) were obviously contradicted by facts. Thus Democritus argued from the falsity of the conclusion to that of the premises :

(6′) There is motion (thus motion is possible).
(5′) The world has parts ; it is not one, but many.
(4′) Thus the world cannot be full.[1]
(3′) The void or (non-being) exist.

So far the theory had to be altered. With regard to being, or to the many existing things (as opposed to the void), Democritus adopted Parmenides' theory that they had no parts. They were indivisible (atoms), because they were full, because they had no void inside.

The central point of this theory is that it gives a rational account of change. The world consists of empty space (the void) with atoms in it. The atoms do not change ; they are Parmenidean indivisible block universes in miniature.[2] All change is due to rearrangement of atoms in space. Accordingly, *all change is movement*. Since the only kind of novelty possible is novelty of arrangement, it is, in principle, possible *to predict all future changes in the world*, provided we manage to predict the motion of mass-points.

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[1] The inference from the existence of motion to that of a void does not follow, because Parmenides' inference from the fullness of the world to the impossibility of motion does not follow. Plato seems to have been the first to see, if only dimly, that in a full world circular or vortex-like motion is possible, provided that there is a liquid-like medium in the world. (Peas can move with the vortices of pea-soup.) This idea, first offered somewhat half-heartedly in the *Timaeus*, becomes the basis of Cartesianism and of the light-ether theory as it was held down to 1905.

[2] Democritus' theory admitted also large block-atoms, but the vast majority of his atoms were invisibly small.

Democritus' theory of change was of tremendous importance for the development of physical science. It was partly accepted by Plato, who retained much of atomism but explained change not only by unchanging yet moving atoms, but also by the ' Forms ' which were subject neither to change nor to motion. But it was condemned by Aristotle who taught in its stead that all change was the unfolding of the inherent potentialities of essentially unchanging *substances.* But although Aristotle's theory of substances as the subjects of change became dominant, it proved barren ; [1] and Democritus' theory that all change must be explained by movement became the tacitly accepted official programme of physics down to our own day. It is still part of the philosophy of physics, in spite of the fact that physics itself has outgrown it (to say nothing of the biological and social sciences). For with Newton, in addition to moving mass-points, *forces* of changing intensity (and direction) enter the scene. True, these changes can be explained as due to, or dependent upon, motion, that is upon the changing position of particles, but they are nevertheless not identical with the changes in position ; owing to the square law, the dependence is not even a linear one. And with Faraday and Maxwell, changing fields of forces become as important as material atomic particles. That our modern atoms turn out to be composite is a minor matter ; from Democritus' point of view, not our atoms but rather our elementary particles would be real atoms—except that these too turn out to be liable to change. Thus we have a most interesting situation. A philosophy of change, designed to meet the difficulty of understanding change rationally, serves sciences for thousands of years, but is ultimately superseded by the development of science itself ; and this fact passes practically unnoticed by philosophers who are busily denying the existence of philosophical problems.

Democritus' theory was a marvellous achievement. It provided a theoretical framework for the explanation of most of the empirically known properties of matter (discussed already by the Ionians), such as compressibility, degrees of hardness and resilience, rarefaction and condensation, coherence, disintegration, combustion, and many others.

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[1] The barrenness of the ' essentialist ' (cf. note 2 above) theory of substance is connected with its anthropomorphism ; for substances (as Locke saw) take their plausibility from the experience of a self-identical but changing and unfolding self. But although we may welcome the fact that Aristotle's substances have disappeared from *physics*, there is nothing wrong, as Professor Hayek says, in thinking anthropomorphically about *man* ; and there is no reason why they should disappear from psychology.

But apart from being important as an explanation of the phenomena of experience, the theory was important in other ways. First, it established the methodological principle that a deductive theory or explanation must ' save the phenomena ', that is, must be in agreement with experience. Secondly, it showed that a theory may be speculative, and based upon the fundamental (Parmenidean) principle that the world as it must be understood by argumentative thought turns out to be different from the world of *prima facie* experience, from the world as seen, heard, smelled, tasted, touched ;[1] and that such a speculative theory may nevertheless accept the empiricist ' criterion ' that it is the visible that decides the acceptance or rejection of a theory of the invisible[2] (such as the atoms). This philosophy has remained fundamental to the whole development of physics, and has continued to conflict with all ' relativistic '[3] and ' positivistic '[4] tendencies.

Furthermore, Democritus' theory led to the first successes of the method of exhaustion (the forerunner of the calculus of integration), since Archimedes himself acknowledged that Democritus was the first to formulate the theory of the volumes of cones and pyramids.[5] But perhaps the most fascinating element in Democritus' theory is his doctrine of the quantisation of space and time. I have in mind the doctrine, now extensively discussed,[6] that there is a shortest distance and a smallest time interval, that is to say, distances in space and time (elements of length and time, Democritus' *ameres*[7] in contradistinction to his atoms) such that no smaller ones are measurable.

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[1] Cf. Democritus, Diels, fragm. 11 (cf. Anaxagoras, Dield fragm. 21 ; see also fragm. 7).

[2] Cf. Sextus Empiricus, *Adv. mathem.* (Bekker) vii. 140, p. 221, 23B.

[3] ' Relativistic ' in the sense of philosophical relativism, e.g. of Protagoras' *homo mensura* doctrine. It is, unfortunately, still necessary to emphasise that Einstein's theory has nothing in common with philosophical relativism.

[4] Such as those of Bacon ; the theory (but fortunately not the practice) of the early *Royal Society* ; and in our time, of Mach (who opposed atomic theory) ; and of the sense-data theorists.

[5] Cf. Diels, fragm. 155, which must be interpreted in the light of Archimedes (ed. Heiberg) II^2, p. 428 f. Cf. S. Luria's most important article ' Die Infinitesimalmethode der antiken Atomisten ' (*Quellen & Studien zur Gesch. d. Math.* Abt. B. Bd. 2, Heft 2 (1932), p. 142).

[6] Cf. A. March, *Natur und Erkenntnis*, Vienna, 1948, p. 193 f.

[7] Cf. S. Luria, *op. cit.*, esp. pp. 148 ff., 172 ff.

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Democritus' atomism was developed and expounded as a point for point reply [1] to the detailed arguments of his Eleatic predecessors, of Parmenides and of his pupil, Zeno. Especially Democritus' theory of atomic distances and time intervals is the direct result of Zeno's arguments, or more precisely, of the rejection of Zeno's conclusions. But nowhere in Zeno is there an allusion to the discovery of irrationals.

We do not know the date of the proof of the irrationality of the square root of two, or the date when the discovery became publicly known. Although there existed a tradition ascribing it to Pythagoras (sixth century B.C.), and although some authors [2] call it the 'theorem of Pythagoras', there can be little doubt that the discovery was not made, and certainly not publicly known, before 450 B.C., and probably not before 420. Whether Democritus knew about it is uncertain. I now feel inclined to believe that he did not; and that the title of Democritus' two lost books, *Peri alogōn grammōn kai kastōn*, should be translated *On Illogical Lines and Full Bodies* (*Atoms*),[3] and that these two books do not contain any reference to the problem of irrationality.[1]

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[1] This point for point reply is preserved in Aristotle's *On Generation and Corruption*, 316a 14 ff., a very important passage first identified as Democritean by I. Hammer Jensen in 1910 and carefully discussed by Luria who says (*op. cit.* 135) of the Parmenides and Zeno: 'Democritus borrows their deductive arguments, but he arrives at the opposite conclusion.'

[2] Cf. G. H. Hardy and H. M. Wright, *Introduction to the Theory of Numbers* (1938), pp. 39, 42, where a very interesting historical remark on Theodorus' proof, as reported in Plato's *Theaeteteus*, will be found.

[3] Rather than *On Irrational Lines and Atoms*, as I translated in note 9 to ch. 6 of my *Open Society* (revised ed.). What is probably meant by the title (considering Plato's passage mentioned in the next note) might, I think, be best rendered by '*On Crazy Distances and Atoms*'. Cf. H. Vogt, *Bibl. Math.*, 1910, 10, 147, and S. Luria, *op. cit.* pp. 168 ff., where it is convincingly suggested that (Arist.) *De insec. lin.* 968b 17 and Plutarch, *De comm. notit.*, 38, 2, p. 1078 f., contain traces of Democritus' work. According to these sources, Democritus' argument was this. *If lines are infinitely divisible*, then they are composed of an infinity of ultimate units and are therefore *all* related like $\infty : \infty$, that is to say, they are all 'non-comparable' (there is no proportion). Indeed, if lines are considered as classes of points, the 'number' (potency) of the points of a line is, according to modern views, equal for all lines, whether the lines are finite or infinite. This fact has been described as 'paradoxical' (for example, by Bolzano) and might well have been described as 'crazy' by Democritus. It may be noted that, according to Brouwer, even the classical theory of the *measure* of a continuum leads to fundamentally the same results; since he asserts that all classical continua have zero measure, the absence of a ratio is here expressed by $0 : 0$. Democritus' result (and his theory of *ameres*) appears to be inescapable as long as geometry is based on the Pythagorean *arithmetical method*, i.e. on the counting of dots.

My belief that Democritus did not know about irrationalities is based on the fact that there are no traces of a defence of his theory against the fatal blow which it received from this discovery. For the blow was as fatal to Atomism as it was to Pythagoreanism. Both theories were based on the doctrine that all measurement is, ultimately, counting of natural units, so that every measurement must be reducible to pure numbers. The distance between any two atomic points must, therefore, consist of a certain number of atomic distances ; thus all distances must be commensurable. But this turns out to be impossible even in the simple case of the distances between the corners of a square, because of the incommensurability of its diagonal with its side.

It was Plato who realised this fact, and who in the *Laws* stressed its importance in the strongest possible terms, denouncing his compatriots for their failure to realise what it meant. It is my contention that his whole philosophy, and especially his theory of ' Forms ' or ' Ideas ', was influenced by it.

Plato was very close to the Pythagoreans as well as to the Eleatic Schools ; and although he appears to have felt antipathetic to Democritus, he was himself an atomist of a kind. (Atomist teaching remained as one of the school traditions of the Academy.[2]) This is not surprising in view of the close relation between Pythagorean and atomistic ideas. But all this was threatened by the discovery of the irrational. I suggest that Plato's main contribution to science sprang from his realisation of the problem of the irrational, and from the modification of Pythagoreanism and atomism which he undertook in order to rescue science from a catastrophic situation.

He realised that the purely arithmetical theory of nature was defeated, and that a new mathematical method for description and explanation of the world was needed. Thus he encouraged the development of an autonomous geometrical method which found its fulfilment in the ' Elements ' of the Platonist Euclid.

What are the facts ? I shall try to put them all briefly together.

(1) Pythagoreanism and atomism in Democritus' form were both fundamentally based on arithmetic, that is on counting.

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[1] This would be in keeping with the fact mentioned in the note cited from the *Open Society*, that the term ' *alogos* ' is only much later known to be used for ' irrational ', and that Plato who (*Repub.* 534d) alludes to Democritus' title, nevertheless never uses ' *alogos* ' as a synonym for ' *arrhētos* '.

[2] See S. Luria, esp. on Plutarch, *loc. cit.*

(2) Plato emphasised the catastrophic character of the discovery of the irrationals.

(3) He inscribed over the door of the Academy: 'Nobody untrained in geometry may enter my house'. But *geometry*, according to Plato's immediate pupil Aristotle as well as to Euclid, treats of incommensurables or irrationals, in contradistinction to *arithmetic* which treats of 'the odd and the even'.

(4) Within a short time after Plato's death, his school produced, in Euclid's *Elements*, a work whose main point was that it freed mathematics from the 'arithmetical' assumption of commensurability or rationality.

(5) Plato himself contributed to this development, and especially to the development of solid geometry.

(6) More especially, he gave in the *Timaeus* a specifically geometrical version of the formerly purely arithmetical atomic theory, that is, a version which constructed the elementary particles (the famous Platonic bodies) out of triangles which incorporated the irrational square roots of two and of three. (See below.) In most other respects, he preserved both Pythagorean ideas as well as some of the most important ideas of Democritus.[1] At the same time, he tried to eliminate Democritus' void; for he realised that motion remains possible even in a 'full' world, provided motion is conceived as of the character of vortices in a liquid. Thus he retained some of the most fundamental ideas of Parmenides.[2]

(7) Plato encouraged the construction of geometrical models of the world, and especially models explaining the planetary movements. Euclid's geometry was not intended as an exercise in pure geometry (as now usually assumed), but as a *theory of the world*. Ever since [3]

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[1] Plato took over, more especially, Democritus' theory of vortices (Diels, fragm. 167, 164; cf. Anaxagoras, Diels 9; and 12, 13); see also the next footnote, and his theory of what we nowadays would call gravitational phenomena (Diels, 164; Anaxagoras, 12, 13, 15, and 2)—a theory which, slightly modified by Aristotle, was ultimately discarded by Galileo.

[2] Plato's reconciliation of atomism and the theory of the *plenum* ('nature abhors the void') became of the greatest importance for the history of physics down to our own day. For it influenced Descartes strongly, became the basis of the theory of ether and light, and thus ultimately, via Huyghens and Maxwell, of de Broglie's and of Schroedinger's wave mechanics.

[3] The only exception is the partial reappearance of arithmetical methods in the New Quantum Theory, e.g. in the electron shell theory of the periodic system based upon Pauli's exclusion principle.

Plato and Euclid, but not before, it has been taken for granted that geometry (rather than arithmetic) is the fundamental instrument of all physical explanations and descriptions, of the theory of matter as well as of cosmology.[1]

These are the historical facts. They go a long way, I believe, to establish my contention that what I have described as the *prima facie* method of philosophy cannot lead to an understanding of the problems which inspired Plato. Nor can it lead to an appreciation of what may be justly claimed to be his greatest philosophical achievement, the geometrical theory of the world which became the basis of the works of Euclid, Aristarchus, Archimedes, Copernicus, Kepler, Galileo, Descartes, Newton, Maxwell, and Einstein.

But is this achievement properly described as philosophical? Does it not rather belong to physics—a factual science—and to pure mathematics—a branch, Wittgenstein's school would contend, of tautological logic?

I believe that we can at this stage see fairly clearly why Plato's achievement (although it has no doubt its physical, its logical, its mixed, and its nonsensical components) was a philosophical achievement; why at least part of his philosophy of nature and of physics has lasted and, I believe, will last.

What we find in Plato and his predecessors is the conscious construction and invention of a new approach towards the world and towards the knowledge of the world. This approach transforms a fundamentally theological idea, *the idea of explaining the visible world by a postulated invisible world*,[2] into the fundamental instrument of theoretical science. The idea was explicitly formulated by Anaxagoras and Democritus [3] as the principle of investigations into the nature of matter or body; visible matter was to be explained by hypotheses

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Concerning the modern tendency towards what is sometimes called ' arithmetisation of geometry ' (a tendency which is hardly characteristic of all modern work on geometry), it should be noted that there is little similarity with the Pythagorean approach since *infinite sequences of* natural numbers are its main instrument rather than the natural numbers themselves.

[1] For a similar view of Plato's and Euclid's influence, see G. F. Hemens, *Proc. of the Xth Intern. Congress of Philosophy* (Amsterdam 1949), Fasc. 2, 847.

[2] Cf. Homer's explanation of the visible world before Troy with the help of the invisible world of the Olympus. The idea loses, with Democritus, some of its theological character (which is still strong in Parmenides, although less so in Anaxagoras) but regains it with Plato, only to lose it soon afterwards.

[3] See the references given above.

about invisibles, about an *invisible structure which is too small to be seen.* With Plato, this ideas is consciously accepted and generalised ; the visible world of change is ultimately to be explained by an invisible world of unchanging ' Forms ' (or substances or essences, or ' natures ' —as we shall see, geometrical shapes or figures).

Is this idea about the invisible structure of matter a physical or a philosophical idea ? If a physicist *acts* upon this theory, that is to say, if he accepts it, perhaps even without becoming conscious of it, by accepting the traditional problems of his subject, as presented by the problem-situation with which he is confronted ; and if he, so acting, produces a new specific theory of the structure of matter ; then I should not call him a philosopher. But if he reflects upon it, and, for example, rejects it (like Berkeley or Mach), preferring a phenomenological or positivistic physics to the theoretical and somewhat theological approach, then he may be called a philosopher. Similarly, those who consciously searched for the theoretical approach, who constructed it, and who explicitly formulated it, and thus transferred the hypothetical-deductive method from the field of theology to that of physics, were philosophers, even though they were physicists in so far as they acted upon their own precepts and tried to produce actual theories of the invisible structure of matter.

But I shall not pursue the question as to the proper application of the label ' philosophy ' any further ; for this problem, which is Wittgenstein's problem, clearly turns out to be one of linguistic usage, a pseudo-problem which by now must be rapidly developing into a bore to my audience. But I wish to add a few more words on Plato's theory of Forms or Ideas, or more precisely, on point (6) of the list of historical facts given above.

Plato's theory of the structure of matter can be found in the *Timaeus.* It has at least a superficial similarity with the modern theory of solids which interprets them as crystals. His physical bodies are composed of invisible elementary particles of various shapes, the shapes being responsible for the macroscopic properties of visible matter. The shapes of the elementary particles, in their turn, are determined by the shapes of the plane figures which form their sides. And these plane figures, in their turn, are ultimately all composed of *two* elementary triangles, viz. the half-square (*or isosceles* rectangular) triangle which incorporates the *square root of two,* and the half-equilateral rectangular triangle which incorporates the *square-root of three*, both of them irrationals.

These triangles, in their turn, are described as the copies [1] of unchanging 'Forms' or 'Ideas', which means that specifically *geometrical* 'Forms' are admitted into the company of the Pythagorean *arithmetical* Form-Numbers.

There is little doubt that the motive of this construction is the attempt to solve the crisis of atomism by incorporating the irrationals into the last elements of which the world is built. Once this has been done, the difficulty of the existence of irrational distances is overcome.

But why did Plato choose just these two triangles? I have elsewhere [2] expressed the view, as a conjecture, that Plato believed that all other irrationals might be obtained by adding to the rationals multiples of the square roots of two and three. I now feel quite confident that the crucial passage in the *Timaeus* clearly implies this doctrine (which was mistaken, as Euclid later showed). For in the passage in question, Plato says quite clearly that '*All* triangles are derived from two, each having a right angle', going on to specify these two as the half-square and half-equilateral. But this can only mean, in the context, that *all* triangles can be composed by combining these two, a view which is equivalent to the mistaken theory of the relative commensurability of all irrationals with sums of rationals and the square roots of two and three.[3]

But Plato did not pretend that he had a proof of the theory in question. On the contrary, he says that he assumes the two triangles as principles 'in accordance with an account which combines probable conjecture with necessity'. And a little later, after explaining that he takes the half-equilateral triangle as the second of his principles, he says, 'The reason is too long a story; but if anybody should test this matter, and prove that it has this property' (I suppose the property that all other triangles can be composed of these two) 'then the prize is his, with all our good will'.[4] The language is somewhat obscure, and no doubt the reason is that Plato lacked a proof of his conjecture concerning these two triangles, and felt it should be supplied by somebody.

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[1] For the process by which the triangles are stamped out of space (the 'mother') by the ideas (the 'father'), cf. my *Open Society*, note 15 to ch. 3, and the references there given, as well as note 9 to ch. 6.

[2] In the last quoted note

[3] In the note referred to I also conjectured that it was the close approximation of the sum of these two square roots to π which encouraged Plato in his mistaken theory. Although I have no new evidence, I believe that this conjecture is much strengthened by the view that Plato in fact believed in the mistaken theory described here.

[4] The two quotations are from the *Timaeus*, 53c/d and 54a/b

The obscurity of the passage had the strange effect that Plato's quite clearly stated choice of triangles introduce irrationals into his world of Forms seems to have escaped notice, in spite of Plato's emphasis upon the problem in other places. And this fact, in turn, may perhaps explain why Plato's Theory of Forms could appear to Aristotle to be fundamentally the same as the Pythagorean theory of form-numbers,[1] and why Plato's atomism appeared to Aristotle merely as a comparatively minor variation of that of Democritus. Aristotle, in spite of his association of arithmetic with the odd and even, and of geometry with the irrational, does not appear to have taken the problem of the irrationals seriously. It appears that he took Plato's reform programme for geometry for granted ; it had been partly carried out by Eudoxus before Aristotle entered the Academy, and Aristotle was only superficially interested in mathematics. He never alludes to the inscription on the Academy.

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[1] I believe that our consideration may throw some light on the problem of Plato's famous 'two principles'—'The One' and 'The Indeterminate Dyad'. The following interpretation develops a suggestion made by van der Wielen (*De Ideegetallen van Plato*, 1941, p. 132 f.) and brilliantly defended against van der Wielen's own criticism by Ross (Plato's *Theory of Ideas*, p. 201). We assume that the 'Indeterminate Dyad' is a straight line or distance, not to be interpreted as a unit distance, or as having yet been measured at all. We assume that a point (limit, *monas*, 'One') is placed successively in such positions that it divides the Dyad according to the ratio $1 : n$, for any natural number n. Then we can describe the 'generation' of the numbers as follows. For $n = 1$, the Dyad is divided into two parts whose ratio is $1 : 1$. This may be interpreted as the 'generation' of Twoness out of Oneness and the Dyad, since we have divided the Dyad into *two* equal parts. Having thus 'generated' the number 2, we can divide the Dyad according to the ratio $1 : 2$ (and the larger section, as before, according to the ratio $1 : 1$), thus generating *three* equal parts and the number 3; generally, the 'generation' of a number n gives rise to a division of the Dyad in the ratio $1 : n$, and with this, to the 'generation' of the number $n + 1$. (And in each stage intervenes the 'One', the point which introduces a limit or form or measure into the otherwise 'indeterminate' Dyad, afresh, to create the new number; this remark is intended to strengthen Ross' case against van der Wielen's.)

Now it should be noted that this procedure, although it 'generates' (in the first instance, at least) only the series of natural numbers, nevertheless contains a *geometrical* element—the division of a line, first into two equal parts, and then into two parts according to a certain proportion $1 : n$. Both kinds of division are in need of geometrical methods, and the second, more especially, needs a method such as Eudoxus' Theory of Proportions. Now I suggest that Plato began to ask himself why he should not divide the Dyad also in the proportion of $1 : \sqrt{2}$ and of $1 : \sqrt{3}$. This, he must have felt, was a departure from the method by which the natural numbers are generated; it is less 'arithmetical' still, and it needs more specifically 'geometrical' methods. But it would 'generate', in the place of natural numbers, linear elements in the proportion $1 : \sqrt{2}$ and $1 : \sqrt{3}$, which may be identical with the 'atomic lines' (*Metaphysics*, 992a19) from which the atomic triangles are constructed. At the same time, the characterisation of the Dyad as 'indeterminate' would become highly appropriate, in view of the Pythagorean attitude (cf. Philolaos, Diels fragm. 2 and 3) towards the irrational. (Perhaps the name 'The Great and the Small' began to be replaced by 'The Indeterminate Dyad' when irrational proportions were generated in addition to rational ones.)

To sum up, it seems probable that Plato's theory of Forms was, like his theory of matter, a re-statement of the theories of his predecessors, the Pythagoreans and Democritus respectively, in the light of his realisation that the existence of irrationals demanded the emancipation of geometry from arithmetic. By encouraging this emancipation, Plato contributed to the development of Euclid's system, the most important and influential deductive theory ever constructed. By his adoption of geometry as the theory of the world, he provided Aristarchus, Newton, and Einstein with their intellectual toolbox. The calamity of Greek atomism was thus transformed into a momentous achievement. But Plato's scientific interests are partly forgotten. The problem-situation in science which gave rise to his philosophical problems is little understood. And his greatest achievement, the geometrical theory of the world, has influenced our world-picture to such an extent that we unconsciously take it for granted.

6

One example never suffices. As my second example, out of a great many interesting possibilities, I choose Kant. His *Critique of Pure Reason* is one of the most difficult books ever written. Kant wrote in undue haste, and about a problem which, I shall try to show, was insoluble. Nevertheless it was not a pseudo-problem, but an in-

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Assuming this view to be correct, we might conjecture that Plato approached slowly (beginning in the *Hippias Major*, and thus long before the *Republic*—as opposed to a remark made by Ross *op. cit.*, top of page 56) to the view that *the irrationals are numbers*, since both the natural numbers and the irrationals are ' generated ' by similar and essentially geometric processes. But once this view is reached (and it was first reached, it appears, in the *Epinomis* 990d–e, whether or not this work is Plato's), then even the irrational triangles of the *Timaeus* become ' numbers ' (i.e. characterised by numerical, if irrational, propositions). But with this, the peculiar contribution of Plato, and the difference between his and the Pythagorean theory, is liable to become indiscernible ; and this may explain why it has been lost sight of, even by Aristotle.

escapable problem which arose out of the contemporary situation of physical theory.

His book was written for people who knew some Newtonian stellar dynamics and who had at least some idea of its history—of Copernicus, Tycho, Brahe, Kepler, and Galileo.

It is perhaps hard for intellectuals of our own day, spoilt and blasé as we are by the spectacle of scientific success, to realise what Newton's theory meant, not just for Kant, but for any eighteenth century thinker. After the unmatched daring with which the Ancients had tackled the riddle of the Universe, there had come a period of long decay, recovery, and then a staggering success. Newton had discovered the long sought secret. His geometrical theory, based on and modelled after Euclid, had been at first received with great misgivings, even by its own originator.[1] The reason was that the gravitational force of attraction was felt to be ' occult ', or at least something which needed an explanation. But although no plausible explanation was found (and Newton scorned recourse to *ad-hoc* hypotheses), all misgivings had disappeared long before Kant made his own important contribution to Newtonian theory, 78 years after the *Principia.*[2] No qualified judge[3] of the situation could doubt any longer that the theory was true. It has been tested by the most precise measurements, and it had always been right. It had led to the prediction of minute deviations from Kepler's laws, and to new discoveries. In a time like ours, when theories come and go like the buses in Piccadilly, and when every schoolboy has heard that Newton has long been superseded by Einstein, it is hard to recapture the sense of conviction which Newton's theory inspired, or the sense of elation, and of liberation. A *unique event* had happened in the history of thought, one which could never be repeated: the first and final discovery of the absolute truth about the universe. An age-old dream had come true. Mankind had obtained *knowledge*, real, certain, indubitable, and demonstrable knowledge—divine *scientia* or *epistēmē*, and not merely *doxa*, human opinion.

Thus for Kant, Newton's theory was simply true, and the belief in its truth remained unshaken for a century after Kant's death. Kant to the end accepted what he and everybody else took for a fact, the attainment of *scientia* or *epistēmē*. At first he accepted it without question. This state he called his 'dogmatic slumber'. He was roused from it by Hume.

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[1] See Newton's letter to Bentley, 1693.

[2] The so-called Kant-Laplacean Hypothesis published by Kant in 1755.

[3] There had been some very pertinent criticism (especially by Leibniz and Berkeley) but in view of the success of the theory it was—I believe rightly—felt that the critics had somehow missed the point of the theory. We must not forget that even today the theory still stands, with only minor modifications, as an excellent first (or, in view of Kepler, perhaps as a second) approximation.

Hume had taught that there could be no such thing as certain knowledge of universal laws, or *epistēmē*; that all we knew was obtained with the help of observation which could be only of particulars, so that our knowledge was uncertain. His arguments were convincing (and he was, of course, right). But here was the fact, or what appeared as a fact—Newton's attainment of *epistēmē*.

Hume roused Kant to the realisation of the near absurdity of what he never doubted to be a fact. Here was a problem which could not be dismissed. How could a man have got hold of such knowledge? Knowledge which was general, precise, mathematical, demonstrable, indubitable, and yet explanatory of observed facts?

Thus arose the central problem of the *Critique*: 'How is pure natural science possible?'. By 'pure natural science'—*scientia, epistēmē*—Kant means, simply, Newton's theory.

Although the *Critique* is badly written, and although it abounds in bad grammar, this problem was not a linguistic puzzle. Here was knowledge. How could we ever attain it? The question was inescapable. But it was also insoluble. For the apparent fact of the attainment of *epistēmē* was no fact. As we now know, or believe, Newton's theory is no more than a marvellous hypothesis, an astonishingly good approximation; unique indeed, but not as divine truth, only as a unique invention of a human genius; not *epistēmē*, but belonging to the realm of *doxa*. With this, Kant's problem, 'How is pure natural science possible', collapses, and the most disturbing of his perplexities disappear.

Kant's proposed solution of his insoluble problem consisted of what he proudly called his 'Copernican Revolution' of the problem of knowledge. Knowledge—*epistēmē*—was possible because we are not passive receptors of sense data, but their active digestors. By digesting and assimilating them, we form and organise them into a Universe. In this process, we impose upon the material presented to our senses the mathematical laws which are part of our digestive and organising mechanism. Thus our intellect does not discover universal laws in nature, but it prescribes its own laws and imposes them upon natures.

This theory is a strange mixture of absurdity and truth. It is as absurd as the mistaken problem it attempts to solve ; for it proves too much, being designed to prove too much. According to Kant's theory, ' pure natural science ' is not only *possible* ; although he does not realise this, it becomes the *necessary result* of our mental outfit. For if the fact of our attainment of *epistēmē* can be explained at all by the fact that our intellect legislates for and imposes its own laws upon nature, then the first of these two facts cannot be contingent any more than the second. Thus the problem is no longer how Newton could make his discovery but how everybody else could have failed to make it. How is it that our digestive mechanism did not work much earlier ?

This is a patently absurd consequence of Kant's idea. But to dismiss it offhand, and to dismiss his problem as a pseudo-problem is not good enough. For we can find an element of truth in his idea (and a much needed correction of some Humean views), after reducing his problem to its proper dimensions. His question, we now know, or believe we know, should have been : ' How are successful hypotheses possible ? ' And our answer, in the spirit of his Copernican Revolution, might, I suggest, be something like this : Because, as you said, we are not passive receptors of sense data, but active organisms. Because we react to our environment not always merely instinctively, but sometimes consciously and freely. Because we can invent myths, stories, theories ; because we have a thirst for explanation, an insatiable curiosity, a wish to know. Because we not only invent stories and theories, but try them out and see whether they work and how they work. Because by a great effort, by trying hard and erring often, we may sometimes, if we are lucky, succeed in hitting upon a story, an explanation, which ' saves the phenomena ' ; perhaps by making up a myth about ' invisibles ', such as atoms or gravitational forces, which explain the visible. Because knowledge is an adventure of ideas. These ideas, it is true, are produced by us, and not by the world around us ; they are not merely the traces of repeated sensations or stimuli or what not ; here you were right. But we are more active and free than even you believed ; for similar observations or similar environmental situations do not, as your theory implied, produce similar explanations in different men. Nor is the fact that we originate our theories, and that we attempt to impose them upon the world, an explanation of their success, as you believed. For the overwhelming majority of our theories, of our freely invented ideas, are unsuccessful ; they do not stand up to searching tests, and are discarded as falsified by experience. Only a very few of them succeed, for a time, in the competitive struggle for survival.[1]

7

Few of Kant's successors appear ever to have clearly understood the precise problem-situation which gave rise to his work. There were two such problems for him, Newton's dynamics of the heavens, and the absolute standards of human brotherhood and justice to which the French revolutionaries appealed, or, as Kant puts it, ' the starry heavens above me, and the moral law within me '. But Kant's starry heavens are seldom identified as an allusion to Newton.[2] From Fichte onward,[3] many have copied Kant's ' method ' and the dreadful style of his *Critique*. But most of these imitators have forgotten Kant's original interests and problems, busily trying either to tighten, or else to explain away, the Gordian knot in which Kant, through no fault of his own, had tied himself up.

We must beware of mistaking the well-nigh senseless and pointless subtleties of the imitators for the pressing and genuine problems of the pioneer. We should remember that his problem, although not an empirical one in the ordinary sense, nevertheless turned out, unexpectedly, to be in some sense factual (Kant called such facts ' transcendental '), since it arose from an apparent but non-existent instance of a *scientia* or *epistēmē*. And we should, I submit, seriously consider the suggestion that Kant's answer, in spite of its partial absurdity, contained the nucleus of a philosophy of science.

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[1] The ideas of this ' answer ' were elaborated in my *Logik der Forschung* (1935).

[2] That this identification is corrected may be seen from the last ten lines of the penultimate paragraph of the *Critique of Practical Reason*.

[3] Cf. my *Open Society*, note 58 to ch. 12

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HANS SELYE AND A UNITARY CONCEPTION OF DISEASE*

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IN most sciences the process of observation and experiment on the one hand, and the development of expositional theory on the other, run on simultaneously. They never entirely coincide, for at any given moment current theory is unable to account for every observation in an inclusive coherent whole ; and at the same time an intrinsically satisfying theoretical presentation may need to remain something of a prediction until observations as yet unmade complete its pattern. Although the two processes run side by side they approach and veer away from each other at different times ; they have different tempos and one or other will predominate at a particular stage in the development of a science. That is, there is a tendency to alternate between periods of rapid advance in the accumulation of new data, made possible by improvements in the technique of observation, and periods of synthesis—or regrouping, to use a military simile—when what has been discovered at the bench or in the field is integrated into a (temporarily) satisfying concept.

At all times, and particularly in the first half of this century, medicine has had much less opportunity than the true sciences for such a stocktaking. To begin with, medicine is not a science at all, but a group of the most highly diverse activities overlapping at productive fringes of experience to form a spectrum ranging from the empiricism of the clinical art itself at one extreme—only in very limited aspects to be regarded as at all approaching a scientific discipline—to the basic sciences, biochemistry, biophysics and the rest, at the other, on which the modern clinician draws increasingly for methods of observing, investigating and treating disease processes. These sciences have lately, through clinical research and work with experimental animals, rewarded him with such an *embarrass de richesses* that he has become less, rather than more, well qualified for the process of theoretical integration. Fighting on so many fronts, and with so many local

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victories, like the successful applications of chemotherapy and radioactive isotopes in daily use, he has had little opportunity to speculate on the possibility that the diverse enemies of the living organism, disease and injury, may use a final common pathway of attack, and may be dealt with by essentially the same mechanism of defence in every case.

If such a satisfactory integration becomes at all possible it will be largely due to the painstaking researches of Hans Selye, Professor of Experimental Medicine in the University of Montreal, and to his development of the concept of the General Adaptation Syndrome and its cognate ' diseases of adaptation.' (It should be noted here that this nomenclature gives rise to some difficulties of interpretation in the light of the classical uses of the words ' syndrome ' and ' disease '; this will be discussed at greater length below.)

To use Selye's own words :[1]

> With the concept of the general adaptation syndrome we have attempted to integrate a number of seemingly quite unrelated observations into a single unified biologic system . . . the keynote of this unification is the tenet that all living organisms respond to stress and that the basic reaction pattern is always the same, irrespective of the agent used to produce stress. We call this response the general adaptation syndrome, and its derailments the diseases of adaptation . . . adaptability and resistance to stress are fundamental requisites for life, and every vital organ and function participate in them.

To discuss in a short space this work, the still evolving product of many years of activity, must inevitably mean presenting Selye's conclusions with a crude dogmatism he would himself disclaim ; and there is, in any case, a vast amount of detail to be added to his picture. But it is possible to sketch the main outlines. The experimental work was based on the initial observation that very similar changes in the adrenal glands and in the blood and tissues of animals could follow the application of many different types of noxious stimulus : physical stimuli such as injury, burning, extremes of temperature ; chemical irritation ; starvation and disturbance of the composition of the diet ; and infective processes of any kind. Also, and this is of particular importance in relation to the problems of psychosomatic disorder discussed later, the same basic response could be elicited by the emotional disturbance resulting from the mere handling or alarming of animals without the introduction of any further agent. This diversity of agents, or stressors, is striking and it must be remembered that each also has its own specific action, e.g. an anaesthetic acts primarily on the central nervous system, cold on the cutaneous blood vessels ; and these specific effects may form the superficial obvious part of the picture, obscuring more or less completely the underlying pattern of the adaptation response which is in all cases the same. In studying a biological response it may be difficult to identify particular manifestations as due to these specific actions, to general damage, or to the nonspecific defence discussed below. Only nonspecific defence reactions are integral parts of the general adaptation syndrome but the specific actions of the eliciting stressors modify the course of the reaction, and other conditioning factors not directly related to the stress situation are heredity, diet and pre-existing disease. But although the point of impact of stress on the organism differs in each case the essential outcome is similar.

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[1] H. Selye, *British Medical Journal*, 1950, 17 June, p. 1383

This outcome is mediated by the endocrine glands, the ductless structures which discharge their hormones directly into the blood stream to influence remotely tissues and organs throughout the body, a controlling mechanism which complements and parallels, though it is more archaic in evolution, the nervous system itself. Familiar examples are the secretion by the pancreas of insulin which enables the tissues to make a complete catabolism of sugar, and the general stimulus to metabolism afforded by thyroxin. In the adaptation syndromes we are concerned with portions of two glands, the pituitary and the adrenal. The pituitary, long regarded as the leader of the endocrine orchestra, is a bilobed organ slung in a pocket in the base of the skull under the cerebral hemispheres. It has intimate nervous and humoral connections with the thalamic and subthalamic regions of the brain, regions associated with the emotions and with the semi-independent vegetative nervous system regulating visceral function and the calibre of small blood vessels. It is the anterior lobe of the pituitary that is now under consideration. The adrenal lies above the kidney on either side ; its medulla has long been known to secrete adrenaline in meeting the urgency of a fight or flight situation, but we are now dealing with the twenty-odd steroid hormones produced by the adrenal cortex, which are the ultimate agents in exciting the complex adaptation response. Anterior pituitary and adrenal cortex are closely linked by hormonal interaction through the bloodstream to constitute a pituitary-adrenal axis which is the main stem of the adaptation response. Apart from its local specific action, the first effect of a stressor, whether this is a burn or an injury, an infection or an anxiety state, is to stimulate the anterior pituitary. (The mediation of this stimulation is not yet clear. Since adrenaline is known from experiment to excite the pituitary in the same way, adrenaline secretion in response to the emergency of stress may be a link, though a further connection between the site of stress and the adrenal medulla would then have to be uncovered.) The anterior pituitary is far from being a simple structure and different cell-groups secrete different hormones such as somatrophin, or growth factor, and thyrotrophin, stimulating the thyroid. The response to stress is the production of adrenal corticotrophic hormone, ACTH, whose function is to reach the adrenal cortex via the bloodstream and stimulate it, in turn, to secrete the corticoid hormones which diffuse throughout the body, everywhere modifying the composition of the blood, lymph and tissue fluids, the disposition of white blood cells, the nature of connective tissue and the structure and function of small blood vessels. Since the function of any organ is largely determined by these factors, notably by changes in its connective tissue ground substance and the behaviour of its vessels, considerable disturbance may result.

In this matter damage and defence go hand in hand; indeed, biologically one definition of stress is the tension represented by the interaction between damage and defence. The adaptation syndrome develops in three stages; the Alarm Reaction, the Stage of Resistance, and the Stage of Exhaustion. The outcome may be complete recovery, or a state of exhaustion, or, and this is here the crux, a prolonged state of disordered adaptation. The characteristic manifestations of the alarm reaction (loosely referred to as shock) are swelling of the adrenal cortex with discharge of secretory granules, increased tissue catabolism, hypoglycaemia, gastro-intestinal erosions, haemoconcentration, involution of lymphoid tissue. Most of these manifestations disappear, or are actually reversed, during the succeeding stage of resistance; but they may reappear when or if the stage of exhaustion is reached, suggesting that the ability of organisms to adapt themselves to environmental change, their 'adaptation energy,' is finite, and possibly dependent on genetic factors.

ACTH is normally a gluco-corticotrophic hormone and induces the formation by the adrenal cortex of *glucocorticoids*, substances such as Compound E (cortisone) and Compound F, which cause increased antibody formation and phagocytosis in the reticuloendothelial system, and the breakdown of protein into sugars; they inhibit wound healing and fibrous tissue proliferation, so antagonising all collagen diseases, and they prevent excessive granulomatous response to infections and irritants. Under certain (ill-defined) conditions ACTH, or a modification, may excite a predominantly mineralocorticoid secretion. The *mineralocorticoids*, such as deoxycortone and desoxocortisone; they are much more potent in controlling salt and water metabolism, favour connective tissue hyalinosis and vascular degeneration, and injure the kidneys so as to produce vascular hypertension. Both groups of corticoids are always produced together in response to stress. It is because of the varying proportions of each group secreted after the stimulus of a normal or disordered discharge of ACTH, the glucocorticoids mainly influencing carbohydrate metabolism and the mineralocorticoids salt and water distribution, that the possibility of disordered as well as ordered adaptation exists.

If the two kinds of hormone are produced in balanced amounts, that is with a preponderance of glucocorticoids, all is well. The stage of resistance is established and will last until the provocative situation ceases to exist or until a supervening stage of exhaustion reproducing many of the features of the alarm reaction ushers in senility or death. There is a self-regulatory mechanism securing that no more ACTH is formed than is required to maintain optimum levels of corticoids in the circulating blood; once adrenal cortical activity has secured this level there is a compensatory suppression of anterior pituitary function, which picks up again as adrenal secretion falls off.

Selye refers to the organs affected by the corticoid hormones as target organs; we may say, in a loose and general way, that these are beneficently affected by glucocorticoids and harmed by mineralocorticoids. In a prolonged and disordered adaptation response, when the action of ACTH is altered in an ill-understood way, a preponderantly mineralocorticoid secretion damages the target organs, the damage being at first a reversible disturbance of behaviour and function, and later an irreversible disturbance based on fixed structural changes; an initial pathological physiology leading to a pathological anatomy. Although the potential target organs and tissues are those of the whole body it is usual for damage to be selective, the brunt of a particular stress falling on certain organs only. This is due to the action of conditioning factors at different levels. Some of these have been already mentioned: the nature of the stress itself, heredity, pre-existing organ disease, the composition of the diet at the time of stress, and the pattern of response to previous stresses. Thus, at one level, excessive protein in the diet favours a mineralocorticotrophic type of pituitary discharge ; and at a lower level sodium increases the sensitivity of the kidneys to overdosage with mineralocorticoids. Locally, chemical irritation augments the fibrosis and hyalinosis these hormones tend in any case to produce. In the ramifying system of stress, where conditioning of some kind is necessary to permit or inhibit target organ damage, it is conceivable that selective conditioning will localise more and more the group of target organs spared or affected, the more nearly the conditioning factor is applied to the ultimate peripheral branches of the system. This selective peripheral conditioning is analogous to the different peripheral consumer use which may be made of additional electricity supplied in emergency to a community, an additional supply which always begins its travels along the same channels but is used in different ways in different districts because there is a local regulation of quality and quantity to meet special needs. The nearer we approach the periphery of such a circuit, the more subject it becomes to conditioning and the more purely local the effects of such conditioning on an increased input originally made available to the whole community.

This is particularly important to the understanding of the diseases of adaptation. It is because conditioning factors can considerably modify the reaction pattern to stress ; because the brunt of response to one and the same stress may fall on the heart in one individual, the kidneys in another, the joints in a third ; and because stresses of the same magnitude excite response in one individual and not in another, that the polymorphism of symptomatology in the adaptation syndrome arises. It would otherwise be impossible to ascribe such apparently diverse diseases as rheumatoid arthritis, allergy, hypertension, to the same causative agent : systemic stress. The concept that such apparently widely differing diseases should follow the same cause is against accepted views on the origin of disease. It is revolutionary for physicians to conceive that the classical catalogue of discrete disease entities may be no more than a continuous spectrum of the possible outcomes in different individuals of the same disordered defence mechanism, itself the product of any one of an enormous number of possible stresses. And yet such an assumption is not entirely without precedent. The manifestations of excessive thyroid activity may be predominantly opthalmic, metabolic or cardiac ; and it is not long since that such overtly dissimilar conditions as consumption, lupus, and Pott's disease of the spine could not possibly be conceived as due to the same cause because their single common factor, the tubercle bacillus, had yet to be discovered.

At this point it may be appropriate to discuss the proper uses of the terms syndrome and disease. Clinically, a disease is a thing in itself, an obvious entity ; scarlet fever is always scarlet fever and acute nephritis is always acute nephritis. It is true that there are boundary zones where one disease may appear to merge into another and may in fact do so, just as sharp species differences in zoology are to a certain extent illusory. But a typical disease speaks for itself, and we recognise it as casually as we recognise the back of a friend in the street. A syndrome, on the other hand, is a constant group of symptoms, a complex which may be the result of quite different pathological causes. Thus the syndrome of sciatica, pain in the distribution of the sciatic nerve and disturbance of reflexes and motor function in the leg, is always the same, but it may be due to an intrinsic neuritis, to a pelvic tumour, or to inflammation or compression of a nerve root in the spinal cord. Dyspepsia, a characteristic group of symptoms of disordered gastric motor, sensory and secretor function, may be due to gastritis, cancer of the stomach, liver disease or anxiety. Although the distinction between syndrome and disease is a convenient one, and the former is usually regarded as a secondary manifestation of the latter, it is also clear that a syndrome is just as much an entity and sometimes more obviously so. Selye's concept would seem to tend to reduce diseases to the category of syndromes relative to the general adaptation mechanism, for they would no longer exist in their own right but as syndrome-like symptom-complexes, organ or system derangements, the numerous chords capable of construction on the adaptation keyboard ; and the present application of the word syndrome becomes colourless and insignificant. It may be questioned then, whether syndrome was an appropriate term for Selye to have applied to the adaptation mechanism itself. Except as ' adaptation syndrome ' might properly be used to describe (in place of the name of the classical disease) the outcome of disordered adaptation in one particular individual, it would seem wrong and confusing nomenclature to apply to the general mechanism, which in a sense is Disease itself, the only and the primary disease to which organisms are subject. A much better term would be the general adaptation process, or reaction.

A constant feature of any disease is that an important part, sometimes the major part, is constituted by the reaction of the tissues to the offending agent. In a bacterial disease such as pneumonia, the inflammatory exudate and other local reactions to the presence in the lungs of toxin-producing pneumococci are responsible for most of the signs, symptoms and disability characteristic of the disease. In some infections the toxaemia is so overwhelming that death occurs without much tissue reaction, as in septicaemia ; in others, there is progressive local tissue necrosis, hypersensitivity and inflammatory change, chronic and disabling. Variable as may be the element contributed to the disease picture by the body's own defence reactions, if it were possible to eliminate this we can imagine that picture to be greatly modified, perhaps the presence of infecting organisms without all that is implied by an infection, and this modification might sometimes be for the better.

Such a therapeutic possibility is implicit in Selye's assumption that the immediate cause of disordered adaptation is an unbalanced secretion of cortical hormones ; for if this imbalance were corrected by administering enough of the right kind of hormone—the glucocorticoids or the ACTH which provokes their secretion—clinical improvement might be expected. This is often the case, and Selye's work, which was an approach purely from the side of experimental pathology, has had a striking confirmation and counterpart in the discovery of the therapeutic effects of cortisone, which has been shown capable of reversing the disease process, the pathological physiology, in many of the very disease processes listed by Selye as due to maladaptation. Just as he grouped a large number of apparently unrelated disorders as due to the common factor of response to stress, so cortisone produces its beneficial results not by combating the infection or injury as such, at all, but by inhibiting the body's reaction to the stimulus ; a reaction often harmful enough to constitute the major part of the disease. It must be added that, in some circumstances where tissue reaction is essential to life, such as the acute inflammation or perforation of a hollow viscus, this inhibition may allow such a process to be silently lethal ; and the regional spread of tuberculosis, which causes intense local tissue destruction and is usually walled off by local fibrosis, may be dangerously facilitated.

It is of considerable theoretical interest to turn now to an aspect of Selye's work which offers stimulating possibilities for the future, that is, its relation to the problems of psychosomatic medicine. That there exists an intimate connection between personality disorder and emotional disturbance on the one hand, and physical disease on the other, has always been implicit, even if not stated and not apparent, for the clinician. It is only during the last twenty years, however, that a systematised school of thought has developed, associated with the names of Flanders Dunbar and Franz Alexander in the United States and James Halliday in this country, which seeks to link causally particular personality types with particular diseases. This might be put more accurately, perhaps, as an attempt to demonstrate both physical and mental disturbance as different aspects of one psychosomatic pattern.

Halliday has recently restated [1] some of the cardinal features of the psychosomatic affections. There is a significant association between the emotionally upsetting event or situation and the emergence or recurrence of the bodily disturbance. There is a disproportion in the sex incidence of these disorders. They are commoner in close relations of such patients, exposed to the same emotional atmosphere, than in the general population ; and the patient who develops one form of psychosomatic disorder is liable to suffer recurrence, or the substitution of another form. As Halliday says : ' the concept of a psychosomatic affection bears resemblance to Selye's concept of stress diseases, but in Western civilisation the common forms of stress are not those of extremes of heat or cold or malnutrition, but emotional, and these stresses are attended by dysfunction of the nervous and endocrine mechanisms which may take a great variety of forms.'

The psychosomatic approach has proved of increasing practical value in the management of such disorders as peptic ulcer, fibrositis, rheumatoid arthritis, asthma and other allergies, and some forms of heart disease, because it adds to the usual physical methods of treatment a purely psychological attack on the personality disorder or disturbance of life situation. However, the situation so developed that increasing acceptance was gained for the concept of psychosomatic disease without any accompanying information as to its mechanism. A study of the life histories of sufferers from these diseases, however convincing as to the relation between physical and mental upheavals, still provided no clues to the link between the two. The ' why ' of the situation became accepted on its obvious merits, but the problem of the ' how ' remained. How could emotional disturbances produce such profound changes, not only in the function but also in the structure of tissues and organs, changes initially reversible and later fixed ? It is precisely here that Selye's work strikes at the hinge between mind and body to provide the beginning of an explanation in terms of the relation between stress and target organs during the process of disordered adaptation. He has shown that emotional stimuli may be equally as effective as physical stress in setting the adaptation mechanism in motion. The pituitary gland is intimately related to the thalamic and subthalamic regions of the brain, and it can be assumed that emotional stress is capable, not only of its well-known property of affecting the blood vessels and viscera through the vegetative nervous system, which has its headquarters in this region, but also of stimulating the anterior pituitary to secrete its corticotrophic hormones, so initiating the adaptation response on the hormonal side. This direct connection between emotion and the formation of cortical hormones is important as knowledge accumulates about the peripheral effects of these hormones. To take a speculative example: mineralocorticoids, produced in excess, are capable of damaging and disrupting the basic ground structure of joints, and if these hormones are produced in response to stress over long periods a possible explanation of such a disorder as rheumatoid arthritis emerges. Similar considerations apply to degenerative changes in the cardio-vascular system and in the kidneys.

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[1] J. Halliday, *Lancet*, 1950, 2 Sept., p. 365.

There is a shift of emphasis from external to internal causal factors in comparing the adaptation response as it is set off by an external environmental factor to the situation when the provocation is emotional. Whatever the possibilities, in experimental animals, of inducing adaptation changes by pure alarm, it does not seem likely that the emotional disturbance of change in environment alone is sufficient in man to do so, and certainly not in the case of the genesis of psychosomatic disorder. In most cases a synthesis must be attempted of classical Freudian views on anxiety situations with Selye's concept. To put the matter very sketchily indeed, the basic anxiety-producing situations are those experienced in early childhood. They remain unconscious and unsolved, retaining a physical basis not only in the cells, but also in the electrical activities, of the cerebrum, something in the nature of a self-maintaining closed circuit. The characteristic feature of the adult neurotic, or of the adult who manages to substitute a psychosomatic disorder for overt neurosis, is that certain features in the current life situation, perhaps quite trivial environmental changes, are capable because of an intrinsic suitability of exciting a quite disproportionate degree of anxiety; they are tuned, as it were, to excite resonance in the closed circuits. It can be seen that the adaptation response might well function in such cases even in the apparent absence of an external stress of valid magnitude, or in the absence of obvious external stress at all, and this certainly seems the case in some patients with rheumatoid arthritis or colitis. From the therapeutic viewpoint, hormones could be expected to produce only temporary remissions in such cases, and this is so, relapse occurring when therapy is discontinued, often with a rebound exacerbation. Formal psychotherapy of the analytic type would seem to be more logical and to offer more to these patients, by allowing the dissolution of the basic disordered cerebral patterns. Whether the adaptation theory, and hormone therapy, are applicable to frank psychosis and psychoneurosis is another matter, and very little is known on the subject apart from the clinical observation that cortisone is frequently capable of eliciting or exacerbating pathological states of mind. It can only be noted, in passing, that in psychoses like schizophrenia a typical bodily configuration suggests that hormonal dysfunction is a constant part of the disease.

It would be improper to conclude without referring to some of the many reservations and criticisms applicable to the theory of the adaptation response. We have not referred at all to the fact that, in addition to the hormonal defence mechanism, there is a parallel nervous defence system which functions from the hypothalamic centres and is itself ultimately hormonal in the sense that it produces its effects by discharges of adrenergic or cholinergic hormones at the periphery; and it may be that more prominence should be given to this. Then again, the existence of two different and opposite kinds of corticoid hormone should not lead to too facile a black-and-white dualism in relating these to the prevention or production of disease. Each is capable of producing the same effects as the other, though characteristically in very different degree, and both are always secreted simultaneously, though in varying proportion, by the adrenal cortex. Both react on and steady the target organ at a particular level of function, disordered or otherwise, just as the tiller of a boat may be held firmly in one of many possible directions by the two tiller ropes adjusted in mutual relation to each other.

A more valid criticism is that, although Selye's work offers an intrinsically satisfying explanation of the observed clinical and experimental facts, the corticoid hormones may not themselves produce the ultimate changes in the target organs, but may merely condition the response of these organs to the action of other, more fundamental, stimuli. In rheumatoid arthritis, for example, it might well be that instead of joint damage resulting directly from mineralocorticoid overdosage, the arthritis is the local manifestation of an antigen-antibody reaction or a hyper-sensitivity state which is due to infection, and that the role of the cortical steroids is merely to determine the manner and magnitude of the response of the joint tissues to such an infection. That is, the schema of the adaptation response might be valid in itself and yet be displaced to a secondary role as a factor conditioning responses to agents other than hormones. The adaptation theory, like many other theories, may be right in linking observed phenomena and predicting others, but for the wrong reasons.

It must be admitted that there is considerable evidence for thinking that the adaptation syndrome of Selye may be no more than just another viewpoint. This does not matter very much so long as it is a fruitful one, as it has turned out to be. For the species the most important rôle of the hormones is reproduction ; but for the individual it is differentiation and adaptation. The gross classical derangements of the endocrinal glands are rare and unimportant in comparison with the hormone derangements resulting from maladaptation to stress. The main fatal syndromes of internal medicine may be included in a list of the results of such maladaptation, the byproducts of faulty accommodation to a variety of non-hormonal pathogenic agents. Though the apparent cause of illness is often infection, exhaustion or old age, failure of the hormonal mechanism might be, if Selye is correct, the commonest cause of death in man. Selye's work is important, not only in the narrow biological field of injury and response to injury, but in relation to the much wider problems of man as a living organism set in the stresses of modern civilisation, so many and so varied and so constant in their impact. It would be only too easy to make facile generalisations about future applications of this work ; but consideration for a moment of man's position as an animal whose physical body is compelled to respond in the same archaic and damaging way to the increasing mental stresses inflicted in modern society, the consideration that the main fatal diseases of internal medicine may be due to maladaptation to such stresses, the possible application of Selye's work in the fields of old age and mental disorder,—all these are of the highest significance to man in his attempt to gain full control over his physical and mental processes.

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THE SCIENTIFIC BASIS OF LEONARDO DA VINCI'S THEORY OF PERSPECTIVE*

M. H. Pirenne

1 *Introduction*

It was during the Renaissance that the basic principles of the science of linear perspective, as applied to representational painting, became fully understood for the first time. Leonardo da Vinci was one of its foremost exponents. For centuries the doctrine of 'Renaissance perspective,' as it is sometimes called, remained generally accepted and understood by those interested in art. In modern times, however, especially since the invention of photography, considerable confusion has arisen concerning its scope and meaning. The relationships between human vision, photography and painting have become the subject of serious misconceptions.

Certain theorists and art historians allege that Renaissance perspective is artificial, suggesting that there may be other systems of perspective which are more 'correct' than Renaissance perspective. In fact, a number of innovators, of whom Hauck[1] in 1879 seems to have been the first, put forward systems of perspective according to which most of the straight lines belonging to the objects to be depicted should be represented in the plane of the drawing by curved lines instead of by straight lines as in Renaissance perspective. These 'curvilinear' systems of perspective are alleged to be superior to the old one because they would reproduce the subjectively curved appearance of straight lines, whereas Renaissance perspective would not.[2]

Professor Panofsky recently stated that 'perspective construction as practised in the Renaissance is, in fact, not "correct" from a purely naturalistic, that is, a physiological or psychological point of view.'[3]

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* Received 22. i. 52

[1] G. Hauck, *Die Subjektive Perspektive und die Horizontalen Curvaturen des Dorischen Styls,* Stuttgart, 1875

[2] M. Zanetti, 'Una proposta di reforma della prospettiva lineare,' *L'Ingegnere,* September 1951, devotes particular attention to curvilinear systems of perspective, showing why these claims are groundless.

[3] E. Panofsky, *The Codex Huygens and Leonardo da Vinci's Art Theory,* The Warburg Institute, London, 1940, p. 106

Panofsky went further, claiming that Renaissance perspective is a 'symbolic form,'[1] which apparently means little more than a system of conventions similar to the forms of versification in poetry. According to such modern views, perspective would be 'primarily a means of integrating design, a new kind of metric system expressing a state of spiritual balance, just as the Alexandrine expressed the balance of seventeenth-century France or the heroic couplet of eighteenth-century England.'[2] These theories are beginning to find a certain degree of acceptance, especially among those who study the historical evolution of methods of depiction in painting.[3]

Leonardo's critics do not generally refute him in detail and on his own ground, and the exact meaning of their own doctrines is not always easy to grasp. In any case, however, it is clear that we must decide between two main propositions: either (A) there is a natural system of perspective corresponding to the way we see; or (B) there is no such system, a number of different artificial systems being perhaps all equally valid, or invalid. Further, if (A) is true, then either (Aa) Renaissance perspective is the natural system of perspective, or (Ab) the natural system differs from Renaissance perspective.

In the present article, I shall try to prove that proposition (Aa) is true. Far from being artificial, Renaissance perspective is the only natural system of perspective. With a high degree of approximation, it corresponds to the way we actually see the world around us. In comparison, rival systems, such as Hauck's 'curvilinear' perspective, are inaccurate. To prove this is to prove that propositions (Ab) and (B) are erroneous and that Leonardo's critics are wrong.

In order to show the philosophical interest of the above controversy, I shall examine briefly a number of points relating mostly to the evolution of painting. Any representational painting or drawing necessarily implies a corresponding system of perspective, be it correct or incorrect, fully developed or rudimentary, consistently applied or not. This is so in spite of the fact that the painter himself may be unable to formulate the theory on which his practice is implicitly grounded. Now assume that Leonardo's doctrine, that is proposition (Aa), is true. Renaissance perspective, and this type of perspective only, corresponds to the way we see the world. It follows that all other systems of perspective must be approximations to, or transformations from, Renaissance perspective. (Military perspective and the system of orthogonal projections used in architecture are such approximations ; the system used in many Chinese paintings is another instance.) It is impossible to make any representational painting without using, wittingly or unwittingly, some at least of the basic principles of Renaissance perspective. Complete rejection of all these principles would make any attempt at representational painting meaningless. These conclusions would have to be dropped if proposition (Ab) or (B) were true.

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[1] E. Panofsky, 'Die Perspective als symbolische Form,' *Vorträge* 1924-25 *der Bibliothek Warburg*, Leipsig-Berlin, 1927, pp. 258-330

[2] 'Appreciating Ucello,' a review of J. Pope-Hennessy, *The Complete Work of Paolo Ucello, The Times Literary Supplement,* 17 November 1950, p. 722

[3] References to authors who base their studies on such views will be found in Panofsky, op. cit. 1940, and in Zanetti, op. cit. 1951. We may quote here Miriam Schild Bunim, *Space in Mediaeval Painting and the Forerunners of Perspective*, New York, 1940; J. White 'Developments in Renaissance perspective,' *Journal of the Warburg and Courtauld Institutes*, 1949, **12,** 58-79; 1951, **14,** 42-69; J. Pope-Hennessy, *The Complete Work of Paolo Ucello,* London, 1950. On the basis of a small number of Leonardo's notes, Mr. White suggests that Leonardo inclined towards a theory of perspective similar to Hauck's. The whole trend of Leonardo's writings runs counter to this suggestion which appears most unlikely when it is realised how mistaken Hauck's views were.

Now independently of whether proposition (Aa), (Ab) or (B) is true, it is evident that a large number of artists, ancient or modern, have produced paintings or drawings which do not conform exactly to, or which deviate strongly from, the laws of Renaissance perspective. Assuming again that Leonardo's theory is true, two alternative explanations may apply here : either (*a*) the painter did not know the principles of perspective or did not know how to apply them ; or (*b*) the painter was versed in the theory of perspective but chose to disregard or to reject some at least of its principles. With regard to (*b*) a painter may fail to apply perspective rules for the sake of artistic aims. Seventeenth-century Dutch painters, such as Vermeer, paid great respect to the laws of perspective, but Dutch tile designers did not. Again, some artists may introduce errors of perspective in their work in order to produce a sort of pastiche of the paintings of archaic artists whose knowledge of perspective was genuinely imperfect. Other painters may disregard perspective rules for deeper and more interesting reasons, which cannot be analysed here. Now if proposition (Ab) or (B) were true, and (Aa) therefore false, the above argument would have to be changed considerably. Assuming, for instance, (Ab) to be true, we should expect at least some painters to have sought the principles of the ' true ' system of perspective rather than those of Renaissance perspective.

In the development of civilisation, from paleolithic times until today, we must expect that there occurred a complicated evolution of man's understanding, or misunderstanding, of linear perspective. Now if proposition (A) is true, there is a system of perspective, be it the Renaissance system or another, which is correct, and thus constitutes the natural foundation of the technique of painting. The study of such an evolution will then be of deeper interest than if, according to (B), perspective were a mere system of conventions. Cave paintings show that men who lived more than 10,000 years ago knew some of the basic principles of ' Renaissance perspective.' If Leonardo was right, these men had already acquired elements of objective knowledge which might be described as scientific.

2 *Leonardo's Window*

Leonardo da Vinci was not the first Italian painter who wrote on perspective. He was conversant with the works of his predecessors, which increases the value of his own writings as an exposition of Renaissance perspective. Leonardo treated the subject in an original manner and delved deeply into its implications. We wish to investigate here only the meaning of the main ideas and theories contained in his note books, without entering into the historical problems of their origin and evolution.[1]

' Perspective is nothing else ' wrote Leonardo ' than seeing a place [or objects] behind a pane of glass, quite transparent, on the surface of which the objects which lie behind the glass are to be drawn. They can be traced in pyramids to the point of the eye and these pyramids are intersected by the glass plane.' [2] Fig. 1 gives a representation of Leonardo's window from a book by Brücke.[3] For the sake of argument the object is taken to be a cube, made of plaster of Paris, of the kind often used in drawing schools.

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[1] *The Literary Works of Leonardo da Vinci*, compiled and edited by J. P. Richter ; 2nd ed. by J. P. and I. A. Richter, 2 vols. (Ital. and Engl.) London, 1939. This edition of Leonardo's writings, which include newly discovered MSS of his *Treatise on Painting*, will be used as a basis for the present discussion. It would appear that a strange fatality prevents Leonardo's *Treatise on Painting* from becoming generally accessible to the public. The above edition is now out of print and libraries in general do not allow it to be borrowed, so that it is hardly accessible to many non-specialists who might wish to read it.

[2] Richter edition, 1939, **1**, 150

[3] E. Brücke, *Bruchstücke aus der Theorie der Bildenden Künste*, Leipsig, 1877. This book seems to be the only modern work in which Leonardo's perspective is discussed at some length from the standpoint of sensory physiology. The figure is reproduced as Fig. 15 of my *Vision and the Eye*, London, 1948, p. 13.

It is easy to repeat Leonardo's experiment. Anyone doing so will notice that the perspective drawing on the glass window does not change when the eye moves freely in its orbit, as it always does in normal vision.[1] Leonardo's experiment requires, however, that only one eye be used, and that the head be kept immobile. The complications which arise when the head is allowed to move, and when both eyes are used, will be discussed in Section 5 below.

This well-known experiment is crucial for the theory of perspective. Its performance should be sufficient to dispel most of the criticisms levelled at Renaissance perspective, without entering into any theoretical discussions. It shows why Leonardo's doctrine remained understood and almost unchallenged for centuries. It may be wondered whether Leonardo's critics have ever cared to repeat it.

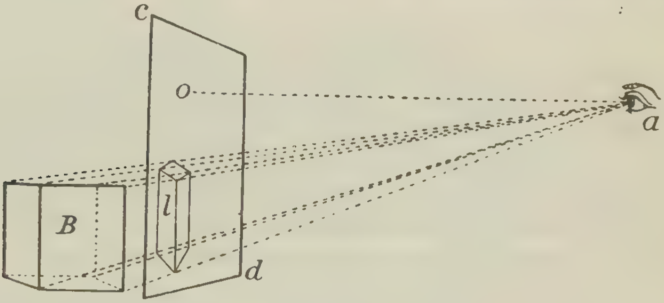

Fig. 1

For Leonardo this experiment was the basic approach to the subject of perspective. Rightly, he adhered to its result in spite of theoretical difficulties. He had no clear conception of the manner in which the eye forms the image of external objects. The dioptrics of the eye only became well understood in the seventeenth century.[2] Leonardo suspected that an inverted image was formed in the fundus of the eye, but he could hardly reconcile himself to this idea. The 'point of the eye,' or centre of perspective, which constitutes the summit of his pyramid of sight, also remained something of a puzzle to him.[3] In the case of an eye kept immobile, the centre of perspective

[1] H. B. Barlow, 'Eye movements during fixation,' *J. Physiol.*, 1952, **116**, 290–306

[2] Cf. C. Scheiner, *Rosa Ursina*, Bracciani, 1630 ; see also *Vision and the Eye*, p. 12.

[3] Richter edition, 1939, Vol. 1, 'Linear perspective'

is, according to Brücke, the anterior nodal point of the optic system. According to Hardy and Perrin,[1] it is the centre of the entrance pupil of the optical system of the eye. These two points are very close to each other. If the eye moves in its orbit, the centre of perspective is, according to Brücke, the centre of rotation of the eye. The distance between this third point and the two others is also so small in comparison to the distance of the eye from the picture, that all three can be treated as one in the practice of painting.[2]

A remark made four centuries later by Helmholtz entirely confirms Leonardo's doctrine. In his *Physiological Optics*, Helmholtz did not deal explicitly with the theory of perspective, which he appeared to take completely for granted. When discussing the difference between uniocular and binocular vision, however, he wrote as follows :

> Suppose that we find ourselves in a room well-known to us and brightly lit. Now, while looking at the room, let us close one eye. We may then believe that we see the room just as clearly and precisely as when we were using both eyes. Yet, under those conditions, we would still get precisely the same visual image if all points of the room were moved so that their distance from the eye were varied at will, while they remained each on its respective line of sight.[3]

As a particular case of the latter part of Helmholtz's remark, we may imagine that all the points of the room move along their lines of sight—which lines thus form Leonardo's pyramid of sight—until they come into the plane of a Leonardo window whose position is chosen at will. According to Helmholtz, then, we would be unable to distinguish the ' painting ' formed in this way on the window from the actual objects in the room behind the window.

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[1] A. C. Hardy and F. H. Perrin, *The Principles of Optics*, New York and London, 1932, p. 465

[2] See Brücke, 1877, note on p. 195. A detailed explanation of the elementary theory of the formation of the retinal image in the human eye, written with a view to explaining Leonardo's system of perspective, will be found in M. H. Pirenne, *Vision and the Eye*, London, 1948. (In the legend of Fig. 15, p. 13 of this book, 'posterior nodal point' should read 'anterior n.p.' to agree with Brücke.) An advanced treatment of the dioptrics of the human eye will be found in Y. Le Grand, *Optique Physiologique*, vol. 1, Paris, 1946.

[3] 'Schliessen wir, während wir das Zimmer betrachten, ein Auge, so glauben wir es nicht weniger deutlich und bestimmt vor uns zu sehen, als mit zwei Augen, und doch würde uns nun genau dasselbe Gesichtsbild gegeben werden, wenn all Punkte des Zimmers so verschoben würden, dass sie ihre Enfermung vom Auge beliebig änderten, aber auf denselben Visirlinien blieben.' H. von Helmholtz, *Handbuch der Physiologische Optik*, 2nd ed., Leipsig, 1896, pp. 609-10.

Under such experimental conditions, that is, using one eye without moving the head, only the state of accommodation of the eye could give the observer information about the distance of the various points which he sees, thus enabling him to distinguish between the real scene and its picture. But it is well known that in general the estimation of distance by such means alone is impossible for distant objects and extremely imperfect for near ones. For example, experiments made by Wundt and reported by Helmholtz [1] show that a human observer is hardly able to give any indication of the absolute distance from his eye of a black thread shown to him against a distant white background. In any case, for all distances larger than 12 metres, the accommodation of the normal eye remains always practically the same independently of the distance.

Thus Leonardo's doctrine is correct. His window is a convenient device to illustrate the principles of perspective, which are embodied in the method of construction based on the pyramid of sight. The picture drawn on Leonardo's window is a central projection of the objects behind the window. The plane of projection is the window plane. The centre of projection or point of view is Leonardo's ' point in the eye,' that is, the centre of perspective defined above.

If the shape and disposition of the objects to be represented are known, geometrical construction makes it possible to draw, on any projection plane and from any point of view, the perspective image of these objects. The science of perspective is merely a part of the theory of central projections. The laws of perspective generally taught to painters refer to a vertical projection plane, the usual case in practice. The plane of projection however need not be vertical. It can even be replaced by a curved surface—as shown by the example of the painted ceiling of the Sistine Chapel.

Once it is seen that the theory of central projections gives the solution to Leonardo's problem, it is not strictly necessary to discuss the theory itself, since no one doubts that, as a mathematical theory, it is correct. Nevertheless it may be worth while to examine a particular case which has often led to misunderstandings, namely the case of a line parallel to the plane of projection. The projection of such a line is parallel to the original line. Consider a long horizontal wall and a plane of projection parallel to it. The outline of the wall on the plane will consist essentially of two horizontal straight lines. In this connection, Jopling[1] in 1835 made the following remark :

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[1] *Physiologische Optik*, 2nd ed., p. 778

> This fact, however, has puzzled several ; for many good draughtsmen are neither mathematicians nor geometricians ; and they have asked, Will not the two horizontal and parallel right lines, for example, forming the top and base of a long wall, appear, to a person who may stand in front of the middle of it, to get nearer and nearer to each other the further they extend both to the right and to the left of the centre ? Have not such lines, therefore, the appearance of curves ? And ought not their representation, when drawn on a plane, as thus viewed, to be curved lines, as there is no appearance of angle in the middle ?
>
> The answer to these questions is, that straight parallel lines on the plane of projection, when viewed from the point of sight, will appear to approach each other ; and also appear as much curved, in proportion to their length, as the long lines of the top and base of the wall would so appear from the point, at a proportional distance, from which they were viewed.

We will return to this argument when discussing subjective visual appearances in Section 4. Suppose now we keep the same point of view, but use another vertical plane of projection which makes an angle with the plane of the wall. On this plane, the wall will be represented by two converging straight lines. These two lines will again give the same subjective appearance as the parallel lines on the first plane, and as the two lines belonging to the wall itself.

This is true provided always the eye remains placed at the correct point of view. If it does not, the appearance of the perspective drawing will differ from that of the actual object ; it will be distorted to a greater or lesser extent.

It is the rectilinear propagation of light which is the foundation of linear perspective. Sunlight falling on an object is scattered in all directions. From every point of the object light rays go forth in an infinite number of straight lines. The air is filled with such rays originating from the lighted objects. At every point of space, rays coming from all the surrounding objects converge and meet. They form ‘ pyramids ’ originating from each object and having as their common summit the point considered. At every point of space therefore the image of the objects is potentially contained in these pyramids. Leonardo proved this by using a camera obscura which he provided, not with one, but with several holes. This camera of course formed as many images of each object as there were holes. Leonardo developed this idea in a most acute manner and returned to it repeatedly in his writings.[1]

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[1] Brook Taylor, *Linear Perspective*, London, 1715. New edition with additions by J. Jopling, London 1835. The remark quoted is found only in the 1835 edition. I am indebted to Dr Zanetti for drawing my attention to it.

The truth of Leonardo's doctrine does not depend in any way on the structure of the eye or of the brain, or on the psychology of the spectator. 'Renaissance perspective' should be valid for animals, even for such animals as insects, whose eyes are built and function very differently from those of man. This was incidentally shown to be true in experiments made on the fruit-fly, *Drosophila melanogaster.*[2] Like many other animals, *Drosophila* gives a typical reflex response to a movement in its visual surroundings. In the experiment, a fly is allowed to creep freely along the horizontal length of a narrow glass cell placed parallel to an illuminated glass plate which carries vertical stripes, black and white. With the plate at rest the fly usually creeps back and forth from one end of the cell to the other. If now the striped plate is moved in the direction in which the fly is creeping the fly turns around and runs off in the opposite direction. Obviously this can occur only when the fly is able to resolve visually the essential elements of the pattern, otherwise the striped pattern looks like a uniform field and its movements must be invisible to the fly. At any illumination there is a critical size of the stripes under which the fly ceases to respond, and the experiments were designed to find this critical size. In the course of these investigations it was found—as was of course to be expected—that the determining factor was not the actual size of the stripes but the visual angle subtended by each stripe at the eye of the fly. For instance, stripes 1.27 mm. wide placed 5.21 mm. from the fly gave the same result as stripes 6.3 mm. wide placed 25.8 mm. away. From the point of view of the fly, the nearer plate with narrower stripes was a projection in 'Renaissance perspective' of the more distant plate with wider stripes. The fly behaved in the same way towards both. Thus linear perspective, as far as such experiments can show, is valid for a fly just as it is for us.

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[1] Richter edition, vol. 1, 'Linear perspective'

[2] S. Hecht and G. Wald, 'The visual acuity and intensity discrimination of Drosophila,' *The Journal of General Physiology*, 1934, **17**, 517-547

3 *Physiological Considerations*

In the quotation above, Leonardo dealt merely with the making of a perspective drawing, without reference to colour and shade. Helmholtz's remarks show how the latter aspects should ideally be taken into consideration. We must apply paint to the drawing and give to each part of the drawing of the cube in Fig. 1, for instance, the same colour and the same brightness as the corresponding part of the solid, real cube. Then every element of the painted surface will send to the eye exactly the same amount of light, and the same kind of light, as would be sent by the corresponding part of the real cube.

It must be emphasised that in many cases, such as sunlit scenes, it is a practical impossibility for the painter to achieve such a result, because the range of light intensities which can be reproduced on canvas or paper is much smaller than the range existing in nature. The expedients used by artists to meet this difficulty are very interesting from the point of view both of the theory of painting and of sensory physiology. A simple example may be given: on account of the effect of irradiation, the painter will generally make the outline of a bright object on a dark background larger than it would be on the basis of purely geometrical optics.[1]

Assuming that such practical difficulties have been surmounted, the eye will receive practically the same distribution of light from the picture as from the real object, since we have seen that differences of depth can hardly be detected under the present conditions. The same retinal image will therefore be formed in both cases, real object or picture. True, the eye moves in saccadic fashion in its orbit when we survey a scene before us, so that for each different position assumed by the eye, the retinal image will be different. This is no objection, however, because in each and every position it will be the same for the object and for its picture. Nervous excitation in the retina and the brain will therefore be the same whether the real object or its picture is looked at, or, to be more exact, the differences will be similar to those which arise when the eye looks at the object itself several times in succession. Accordingly, the observer will receive the same subjective impression when he looks at the painting as when he looks at the real object.[1]

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[1] Brücke discusses these problems in the book quoted, and so does Helmholtz in his paper, 'On the Relation of Optics to Painting,' *Popular Lectures on Scientific Subjects*, 2nd series, London, 1903. Photographs rarely give a good rendering of such effects, because these effects depend on special properties of the human eye. Painting may be more accurate than photography in reproducing the visual appearance of scenes and objects. Cf. p. 179, n. 2.

The retinal image, spread over the concave fundus of the eye, is evidently different from the real object, the cube for instance, which is a solid. It must be emphasised that it is also different from the picture on Leonardo's window. For instance, the images of the horizontal lines representing the long wall used as an illustration to Topling's remarks will, *on the retina*, always tend to converge at both ends, as the visual angle they subtend becomes smaller and smaller. An obvious difference also is that the retinal image is upside down compared to the picture.

We have seen that the plane window could have been replaced by a curved surface of projection, for instance by a surface resembling the retina. Even then, however, and neglecting the inversion of the retinal image, the shape of the two images would not be identical, on account of the characteristics of the optical system of the eye. Similarly a photographic camera does not necessarily form a picture which is exactly the same as the perspective projection on a plane Leonardo window.[2] The aim of most camera designers is to produce a picture conforming to Renaissance perspective. But it was not the *primary* aim of Leonardo to produce a picture identical with that given by a *camera obscura*.

The light pattern formed on the retina, for any particular position of the eye, stimulates nerve endings sensitive to light and thus determines a pattern of excitation in the optic nerve. This pattern is propagated through the visual pathways, undergoing modifications on the way, and eventually reaches a certain area of the cerebral cortex, at the back of the brain. The pattern of stimulation may therefore be considered at various levels from optic nerve to cortex. At all these levels it will remain different in shape from Leonardo's picture, and from the real cube itself. Let us examine the situation in the visual cortex. There is a definite functional correspondence between retina and visual cortex, but this is not a simple proportional relationship. The cortical region corresponding to a given area in the central part of the retina is markedly larger than the cortical region corresponding to an area of similar size in the retinal periphery. Further, the visual cortex is a layer of nerve cells whose shape is very different from a fragment of a plane or of a sphere. It extends over both cerebral hemispheres, which are separated by a deep commissure. In each hemisphere it extends over several brain convolutions separated by folds. Thus the pattern of stimulation formed in our cerebral cortex when we look at the cube or at Leonardo's picture will be quite different in shape from either the cube or the picture, although it will still bear some resemblance to them.[1] Little is known of the stimulation patterns which follow after the cortical level, but it is safe to conclude from all this that the shape of the stimulation pattern found at the various levels of the nervous system never resemble closely the shape of either Leonardo's picture or of the real object itself. It must also be borne in mind that the patterns of excitation, from retina to cortex, will change several times per second on account of the saccadic movements of the eye.

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[1] It is interesting to note that in his *Principles of Gestalt Psychology*, London, 1935, p. 79, K. Koffka seems to agree fully with the present argument.

[2] The lens of a photographic camera must be particularly well corrected if it is to yield a photograph which, after compensating for the inversion of the image, constitutes an exact perspective projection of the objects whose picture was taken. Even well-corrected lenses may not satisfy all the conditions necessary for giving such a photograph. The (generally unpractical) pin-hole camera is better suited to this purpose. See Hardy and Perrin, *Optics*, p. 466. The same authors, on p. 468, illustrate with a photograph the principle underlying any reproduction of space on a plane surface, namely, that the reproduction is correct in its perspective relationship only when it is viewed from a particular distance and in a particular position, that is, from the point of view or centre of perspective.

Thus it is true that a complicated relationship of a peculiar kind exists between the picture on Leonardo's window, on the one hand, and the stimulation patterns in the retina and other parts of the nervous system, on the other hand. But this picture and these patterns are different in shape. Further, it cannot be too much emphasised that the picture drawn in perspective does not by any means aim at representing the retinal image or any pattern of nervous stimulation. Its aim is to send the eye the same distribution of light as that which the object itself would send. It is for this reason that the problem of perspective is one merely of geometrical optics, not of neurophysiology. Physicists will recognise here the kind of situation in which the human eye is used as a 'null instrument,' as for instance when matching visually the two halves of a photometric field filled with the same kind of monochromatic light. The characteristics of the retina and of the brain do not affect such matches ; they remain valid when the original observer is replaced by another, or even when he is replaced by a purely physical detector of light such as a photocell.

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[1] For details and bibliography concerning these problems of visual anatomy and physiology, see e.g. *Vision and the Eye.*

4 *Subjective Visual Appearances*

We have so far considered the real object, that is, for instance, the cube of Fig. 1, and its retinal image, with the physiological patterns of excitation which occur in the nervous system. We must now consider the subjective visual appearance of the real object. It is a fact that lines which are geometrically straight may subjectively appear curved to a human observer.[1] This has led some to the false conclusion that a straight line should be drawn as a curve on Leonardo's window. Since this is an error which lies at the root of many misunderstandings it is necessary to examine the bearing of subjective visual appearances on the theory of perspective.

The subjective appearance is what I see when I look at the object. But *by construction* the picture on Leonardo's window has been drawn in such a way that what I see when I look at the picture is the same as what I see when I look at the object. That is, the subjective visual appearance of the object and of the picture are the same. Leonardo's doctrine therefore remains entirely unaffected, as the example of the subjective curvature of straight lines will show.[2]

Returning to Fig. 1, we see that a straight line belonging to the real object (I), for instance one of the edges of the cube, is represented in the perspective picture (II) by a straight line, or in exceptional cases by a single point. A straight line of (I) is never represented by a curve in (II). Now to a given observer the straight edges of the cube (I) may subjectively appear curved. But then the corresponding lines of the picture (II) will also appear curved to him. *If the lines in* (II) *had been drawn curved they would not have the same subjective appearance as the lines of the real object* (I) : their apparent curvature might be too strong or too weak compared with (I) ; they might even appear straight. However, since the straight lines of the picture (II) have been drawn so as to be exactly superimposed on the straight lines in (I), they will appear curved in (II) as in (I). They will appear curved in exactly the same way in (II) as in (I). If to another observer the edges of the cube (I) do not subjectively appear curved, but straight, the lines of the picture (II) of course will also look straight. Thus Renaissance perspective reproduces subjective curvature, or the lack of subjective curvature, accurately.

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[1] See e.g. Helmholtz, *Physiologische Optik*, 2nd ed., p. 695 ; Lord Charnwood, *An Essay on Binocular Vision*, London, 1950

[2] A complete discussion of this subject will be found in Zanetti, *L'Ingegnere*, 1951.

Any other system of perspective, for instance a system according to which the edges of the cube are drawn as curved lines on the window, will give on (II) a picture different from Leonardo's. This means that the lines in this new picture will not be seen superimposed on to the corresponding lines of (I). They will therefore not reproduce the subjective appearance of (I) accurately. This demonstrates that Renaissance perspective is the *only* system of perspective which reproduces exactly the subjective curvature of straight lines or, by generalisation, of any lines.[1]

The picture (II) in the above experiment bears to the object (I) the same kind of relationship as that which exists between a design (i) consisting, say, of a circle drawn on a piece of paper and a copy (ii) of this design made by tracing. A good tracing (ii) will give an adequate representation of the original design (i) for each and every observer, independently of his physiological or psychological make up. Let us assume for the sake of argument that a man looking at the circle (i) sees a certain ' visual image ' which might be described as a peculiarly-shaped, irregular ellipse. Now replace the original (i) by the tracing (ii). The observer of course will again see precisely the same ' visual image ' consisting of the same ellipse with exactly the same irregularities in its shape. When looking at the circle, another man Y may see a ' visual image ' different from X's. But in spite of this Y, like X, will see exactly the same ' visual image ' when looking at the tracing or at the circle. In fact there is no need whatever to introduce ' visual images,' which are hard to define or to describe, when discussing a tracing. The only thing that matters is that the tracing and the original must, for all observers, *look the same*. What they look like does not enter into the argument. The situation is essentially similar in the case of the perspective picture of an ordinary subject. The reader will have noted that the case of the tracing may be considered as a particular case of linear perspective, in which the object to be depicted lies in the projection plane itself. If curvilinear perspective were correct, therefore, the tracing of a straight line should apparently be a curve ![2]

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[1] Cf. the quotation from Jopling's edition of Brook Taylor's *Linear Perspective* in Section 2 above.

[2] It will be obvious that the problem of the curved lines in Greek architecture

From the preceding sections it follows that the picture drawn in perspective is neither a subjective visual appearance, nor a pattern of excitation in the brain, nor a retinal image of the object. It is simply a device to send into the eye the same light distribution as would be sent by the object itself.

5 *Movements of the Head. Binocular Vision*

Returning to Leonardo's window, we have seen that eye movements do not affect the result of the experiment. This is due to the fact that for each possible orientation of the eye considered in turn, the retinal images produced in a given observer either by the object (I) or its picture (II) will be the same. If, using one eye, we move the head, however, the picture (II) will no longer be seen superimposed on the object (I), and will appear distorted in comparison with (I). As stated in Section 2, the perspective drawing is fully accurate only for one position of the centre of rotation of the eye, namely the centre of perspective from which it has been drawn.[1]

A worse complication occurs when both eyes are used. As Leonardo knew, it is impossible to make a flat painting giving the same illusion of depth in binocular vision as can be obtained in uniocular vision when keeping the head still. We need here two different perspective pictures, one for the right eye only, the other for the left eye only. These cannot be drawn together on the window in the ordinary way. They must be viewed separately with the use of a stereoscope or some similar device. When seen with both eyes, therefore, the simple painting made according to Renaissance perspective cannot give the same accurate representation of the real objects as it can when seen with one eye. Yet Renaissance perspective remains a fairly good approximation, and, more important, it is probably the best possible approximation. Replacing Renaissance perspective by another system of perspective, such as one using curved lines to depict straight lines of the real object, will in general

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is quite distinct from the present problem. The aim of Greek architects may, or may not, have been to give the observer a subjective appearance of rectilinearity by the use of curved lines. But in any case it cannot have been to make a representation of lines which are objectively and geometrically straight, for architecture is not a representational art.

[1] Cf. p. 179, n. 2

introduce additional errors and distortions, thus making matters worse instead of improving them.

In any case, it is easy to see, by shutting alternately one eye and the other, that the perspective projections on Leonardo's window for the right and for the left eye do not differ greatly when all the objects seen are at a distance of at least a few yards. One of the differences is a general displacement of the perspective image for the right eye relative to that for the left eye. The magnitude of this displacement decreases if the distance of the eye from the window increases. Practical rules concerning the distance of the point of view from the painting and the size of the painting tend to minimize the effects of binocular vision as well as the distortion which occurs if the painting is not seen exactly from the point of view. Stereoscopic vision is, however, very accurate in detecting relief, or the lack of relief, so that in general the flatness of the canvas or of the wall on which the painting is made remains obvious when both eyes are used. The use of systems of perspective different from Leonardo's would not obviate this defect.

6 *Conclusions*

The linear perspective of the Renaissance is a scientific doctrine which agrees with the physiology and psychology of vision and rests on the fact that light rays travel in straight lines from the painting to the eye of the observer. Whilst Renaissance perspective is unavoidably imperfect because we generally use not one eye, but both eyes when we look at paintings, such imperfections cannot be remedied by using the rival systems of perspective which were put forward with a view to superseding Renaissance perspective. These new systems would introduce additional errors and distortions.

The Renaissance system of perspective is the only one capable of reproducing adequately the subjective curvature of straight lines in outer space. Its validity is unaffected by the fact that our retina is curved. It is independent of the structure of the eye and the nervous system and of the psychology of the observer. Renaissance perspective is valid for the eyes of such animals as insects as it is for ours. The sciences of astronomy and surveying are made possible by this fact, which has also been used from time immemorial by craftsmen who look along the edge of a wall or a beam to verify that this edge is a straight line.[1] The suggestion that perspective has changed with the evolution of the mind of man is untenable. It is the understanding of the science of perspective by man, and its methods of application, which have developed during the course of human prehistory and history. 'The strange fascination which perspective had for the Renaissance mind'[2] was the fascination of truth.[3]

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[1] It is unnecessary to insist on the fact that we are dealing here with the space of everyday experience, the space of the craftsman, the architect and the surveyor, and that for all practical purposes this is Euclidean space. It was with this space that Leonardo was concerned. R. K. Luneburg in *A Mathematical Analysis of Binocular Vision*, Princeton, 1947, attempts to give a mathematical expression to a number of subjective phenomena, such as the subjective curvature of straight lines. The subjective space he defines is a mathematical concept which is not meant to correspond to the space of practical life.

[2] Panofsky, *The Codex Huygens*, p. 106

[3] I am greatly indebted to Dr A. C. Crombie for discussions and help in writing this article.

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The Principle of Methodological Individualism

In the May issue of this *Journal* I based a discussion of explanations in social science and in history on the principle of methodological individualism. (The terms ' social science ' and ' history ' are taken to exclude the study of purely physical, biological, or behaviouristic aspects of human groups.) This principle states that social processes should be explained by being deduced from principles governing the behaviour of participating individuals and from analyses of their situations, and *not* from super-individual, ' holistic,' sociological laws.

I have recently attended to a good deal of criticism of this principle. In particular, its status has been queried. Is it analytic ? If so, is it significant ?

When I wrote the article I did believe that the principle was both significant and analytic. I considered it (and still consider it) significant, because it rules out a good deal of Plato, a certain amount of Aristotle and Marx, and much of Comte, Durkheim and Malinowski. I believed that it was analytic for three reasons : (1) I held (and still hold) that whereas physical things can exist unperceived, social ' things ' like laws, prices, prime ministers and ration-books, are created by personal attitudes. (Remove the attitudes of food officials, shop-keepers, housewives, etc., towards ration-books and they shrivel into bits of cardboard.) And I supposed that this truism entailed methodological individualism : if social phenomena are generated by individuals they can only be explained individualistically. (2) It is a fact that the social scientist and the historian have no ' direct access ' to the overall structure and behaviour of a system of interacting individuals (in the sense that a chemist does have ' direct access ' to such overall properties of a gas as its volume, pressure and temperature, which he can measure and relate with no knowledge of gas-molecules). It is also a fact that the social scientist and the historian can often arrive at fairly reliable opinions about the personalities, beliefs and intentions of individuals. And I supposed that these two facts together entail that a theoretical understanding of an abstract ' social structure ' can only be derived from more empirical beliefs about concrete individuals. (3) Methodological individualism appeared to me to be a rule which nothing would lead me to abandon.

I no longer believe that methodological individualism is *entailed* either by the truism that social objects are created by personal attitudes or by the 'invisibility' of social structures (though I believe that these two considerations *support* methodological individualism). And I now believe that under extremely unlikely circumstances I would hold methodological individualism in abeyance, demoting it from a rule to an aspiration.

The investigator of a system of interacting components would have abandoned methodological individualism if he did not believe that the system's overall behaviour could be deduced from (*a*) principles governing the behaviour of its components, and (*b*) descriptions of their situations; or, to put it positively, if he believed that the behaviour of its components could be deduced from (*a*) macroscopic laws which are *sui generis* and which apply to the system as an organic whole, and (*b*) descriptions of the positions (or functions) of the components within the whole. This belief constitutes methodological holism.

(1) It is now clear to me that the mere fact that prices, for example, are charged and paid by people, that they are human creations, does not, by itself, entail that the whole price-system may not be governed by an overall law which is underivable from propositions about individual behaviour. (2) It is also clear to me that the 'invisibility' argument does not entail methodological individualism. For a holist who denied that 'the English State, for example, is a logical construction out of individual people,'[1] and who asserted that it is an organism which develops, and responds to challenges, according to holistic laws, might also admit that only its individual components were visible and that any operational definition of the laws it obeyed would be in terms of individual behaviour. (Thus he might claim that 'The English State aims at self-preservation' is literally true and not a shorthand statement about English people, while admitting that it could only be tested by observing their behaviour.) (3) I now believe that methodological individualism in the social sciences might conceivably have to be put into abeyance. For it does apparently have to give way, provisionally at least, to methodological holism in the case of certain non-human social systems.

Consider three different systems of interacting components: (*a*) the solar system as conceived by classical mechanics; (*b*) the economic system as conceived by Adam Smith; (*c*) a beehive.

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[1] A. J. Ayer, *Language, Truth and Logic*, 2nd. ed., London, 1948, p. 63

(*a*) Here, methodological individualism is altogether valid. The behaviour of the whole system can be explained by applying the inverse square law and the law of inertia to the system's components, if their relative positions, masses and momenta are known. Indeed, methodological individualism in the social sciences is analogous to the method of resolution and re-composition which characterises Galilean and Newtonian physics : the method, namely, of analysing a complex situation into its atomic constituents and into the simplest principles which they obey, and of deductively reconstructing the whole situation from these.

(*b*) Adam Smith stated that the individual

> generally, indeed, neither intends to promote the public interest, nor knows how much he is promoting it . . . ; by directing [his] industry in such a manner as its produce may be of the greatest value, he intends only his own gain, and he is in this, as in many other cases, led by an invisible hand to promote an end which was no part of his intention. (*The Wealth of Nations*, Bk. 4, Ch. 2.)

But the invisible hand is, strictly, gratuitous and misleading ; for what Smith actually showed was that individuals in competitive economic situations are led by nothing but their *personal dispositions* to promote, unintentionally, the public interest. Here again, methodological individualism is altogether adequate.

(*c*) Mr E. S. Russell, basing himself on experiments by Rösch, has reported the following strange fact [1] (strange, that is, to the methodological individualist). If young worker-bees (whose normal function is to feed the larvae from their salivary glands) are segregated into one half of a hive sealed off from the other half, into which have been segregated the older worker-bees (whose salivary glands have atrophied and whose normal function is to produce wax from their newly developed wax-glands, and later to forage), then the following will occur : after two days' dislocation and near-starvation some of the young workers will start foraging and their salivary glands will atrophy prematurely ; while the atrophied salivary glands of some of the older workers will revive and continue functioning long after the normal period, enabling them to feed the larvae in their half of the hive. The bees' functions will be increasingly differentiated until the division of labour in both halves approaches that of a whole hive.

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[1] In this *Journal*, 1950, **1**, 113–114.

Here, it really is as if individual bees were led by an invisible hand, not merely to promote the interests of the whole half-hive, but to adapt drastically their biological structure in order to do so. It seems extremely difficult to believe that the emergence of these two new systems of specialised functions could be explained individualistically, in terms of the situations and principles of behaviour of each bee, because all the bees in either half of the hive were of approximately the same type and in approximately the same situation, yet only the requisite number adapted themselves to new functions. Thus the bee-hive appears to be an organism in the sense that its pattern of behaviour is determined by teleological principles which apply to the hive as a whole and which cannot be derived from a knowledge of individual bees.

The principle whose status I have been trying to elucidate is a methodological rule which presupposes the factual assertion that human societies are not organisms in the above sense. There is no evidence to suggest that this presupposition is false. But it is at any rate conceivable that such evidence will be found. If this happened I should not wholly abandon the principle of methodological individualism ; for *if* holistic sociological laws were discovered I should hope that they were not *sui generis*, but were themselves explicable in individualistic terms (just as I hope that the re-emergence of specialisation in the bifurcated bee-hive will eventually be explained individualistically). However, the rule of methodological individualism *would* have to be partially suspended in the improbable event of a sociological discovery which undermined the rule's factual basis.

J. W. N. WATKINS

The Mechanical Chess-Player

ASHBY'S[1] discussion of the mechanical chess-player to some extent anticipates a paper of my own[2] now in the press. May I be permitted to quote a few of my results, and raise a question of terminology ? The quantity measured in 'bits' is negative entropy. This can be used as a measure of information, control, design (Ashby), specification, complication (von Neumann[3]), or diathesis (Kapp[4]).

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[1] W. R. Ashby, this *Journal*, 1952, **3**, 44

[2] J. B. S. Haldane, *Sankhya* (in press)

[3] J. von Neumann, *Cerebral Mechanisms in Behaviour*, London, 1951

[4] R. O. Kapp, *Mind, Life and Body*, London, 1951

I have deliberately used ' specification ' where Ashby uses ' design ' because, like him, I hold that it can arise through the action of a conscious designer, by unconscious biological processes such as ' heredity,' or by random processes plus selection. I suggest that such a neutral word may prove more useful than ' design,' as ' function ' is often more useful than ' purpose ' in biology.

I have tried to estimate the amounts of specification in various organisms from genetical data. The numbers of ' bits,' or binary units, range from the order of 100 in a bacteriophage to somewhat under a million in a mammal. Besides the hereditary specification, an organism receives and emits information during its life, and may store some of it. The amount stored by a man is certainly many million units, but could be under 10^9. The human output of control can reach 50 units per second during skilled work. Given a primitive ocean lasting for about 10^9 years and containing a variety of organic molecules, the amount of specification which could reasonably be expected to arise by a random process is of the order of 100 units (very definitely more than 50 and less than 200), apart from the considerable further amount embodied in metastable organic molecules.

I agree with Ashby's treatment of natural selection, and describe it (along with certain processes which probably occur in nervous systems) as a method for the conversion of noise into message.

One further point is, perhaps, not irrelevant to the question posed by Ashby. Some human limitations are due to the shortness of our lives, as others are due to the relative simplicity of our sensory and motor organs, and probably to the relative simplicity of our cerebral structure. A machine has calculated π to more than a thousand decimal places. It is doubtful whether a man could do so with pencil and paper in a lifetime. It would, I believe, be quite practicable to make a machine which could beat any chess champion *provided the champion had to make his or her decisions as fast as the machine.* Ashby's homeostat effectively solves sets of four linear differential equations accurately to one or two significant figures, and does so much more quickly than Ashby can do it. It cannot solve a system of five, nor can it solve even one accurately to five significant figures. It would be quite easy to arrange for a ' game ' between Ashby (A) and his homoeostat (H) as follows :

A ' dealer ' provides each with a set of 16 random numbers forming a matrix. A and H each have to ' answer ' as quickly as possible whether it has a latent root with a positive real part, the score being adjusted so that a random series of answers by A, who, unlike H, knows the probability of a 'success', would score zero on the average. Each game stops when A or H has answered. No doubt A would sometimes win, when the matrix was of a very simple character, but in the long run I would back H. If I am correct A has answered his question not only in theory but in practice.

This does not prove that A has more 'perfection,' to use the Cartesian word, than H, except the rather limited kind involved in being able to do certain operations more quickly. This is the kind of 'perfection' which we grade, not merely in racing, but to a large extent in examinations and intelligence tests. A quantitative discussion of the 'abilities' of different types of machine which included their time parameters might throw some light on the assessment of human abilities.

It would also make nonsense of the supposed antithesis between chance and design in cosmology. If Brahma, though not eternal, has a time scale of about 10^{15} of our own, his easiest method of creating intelligent beings like ourselves might be to leave a few planets at a suitable distance from their suns for $2 \cdot 10^9$ years, as we light a match with confidence that one of a large number of random events will usually set off a chain of reactions. This is not intended as an argument for theism, but merely as a plea for precision in the discussion of that hypothesis.

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Mind-Like Behaviour in Artefacts and the Concept of Mind

In his note (this *Journal*, February 1952) on Dr D. M. MacKay's paper 'Mind-Like Behaviour in Artefacts' (August, 1951), Dr M. H. Pirenne makes the point that a close study of the scope and meaning of the laws of physics is necessary if we are to solve the problem of free-will versus mechanism. I agree with him that the problem largely stems from the meaning we give to these concepts. Nevertheless, Dr Pirenne appears to be confident of the validity of physical theory in the field of human behaviour ; we are unlikely he tells us, to succeed in our task by denying the validity of these laws. He criticises Professor G. Ryle's assertion in *The Concept of Mind* that whilst a game of billiards is governed in each of its states by the physical laws of motion, these laws do not ordain the course of the game, since the players decide what strokes they will make. Dr Pirenne asserts that

Professor Ryle invents a new kind of physics which is inapplicable to the bodies of man.

However, if I understand Professor Ryle in this matter, he is not constructing a new physics, but merely pointing out that although the individual player decides the kind of stroke he makes, once the choice is made the course of the game may be described in terms of physical laws. Professor Ryle seems to be comparing an ex-post description of the game, with our ability to decide for ourselves the lines on which the game should proceed. He believes that there is no contradiction involved in saying that two distinct principles apply to the same process.

Dr Pirenne, after contrasting Professor Ryle's view with that of Laplace where everything in the Universe, including our bodies, is determined by physical laws, asserts that ' an examination of physics, *qua* physics, gives no indication that the new physical laws should not apply to the components of the human body, just as Laplace's Laws did ' (p. 316). But whether we deal with Newtonian physics or quantum mechanics, as pieces of physics, they were never intended to tell us anything about human behaviour. Dr Pirenne seems to believe that there is nothing in these laws to prevent them from being applied to the components of human bodies, yet he does not indicate whether they apply to segments of human behaviour as well as electrons, molecules, bones, sinews and muscles. And if, as the quotation from Professor Dingle suggests, physical laws are deductions from experience, are these laws also applicable to its more qualitative aspects (such as our ' consciousness of freedom ') from which they have been expressly excluded? It seems unlikely from Dr Pirenne's other remarks that he would subscribe to such a view.

If we were to restrict ourselves to a description of the physical properties of bodily states, this would not give us much insight into the way people behave. Human behaviour has little significance without some reference to psychological motivation. Knowledge of the probability distributions of electrons would, for example, be of little avail to a general in the field who wished to predict what the opposing commander's next move was going to be. This does not involve a denial of the validity of physics in the field of human behaviour, but merely indicates that there are limits to its applicability, and that it requires to be supplemented by reference to motives. Perhaps, after all, this is the point Dr Pirenne is trying to make.

When Dr MacKay informs us that his artifice shows ' randomness ' in the domain which in a human being is that of free-will, and that behaviourally there is no reason why the two should be distinguishable, he fails to take into account the part played by motivation ; if it is left out of the picture, human behaviour is reduced to an aimless unorganised activity.

Further, the word ' randomness ' is ambiguous ; we need to distinguish between (1) its popular everyday usage, (2) its mathematical meaning. In (1) we concern ourselves with the cause and not merely the result of a performance, as when we refer to an event which occurs by chance or accident, and contrast it with events brought about by design or purpose. In this case, ' randomness ' has a causal significance. In (2) as used in statistics, we are primarily concerned with the result obtained, with certain irregular series, but not with their origin. The productive agency may vary, it may be conscious design or a complicated interaction of many causes. A random distribution in itself tells us nothing about its origin ; e.g. the digits in the development of π, may be taken as illustrating a random series, yet the position of every digit is necessarily determined. ' Randomness ' may also be simulated as when we deliberately shoot at a target so as to produce a random distribution of hits.

When it is suggested that a machine can simulate free-will, by having a random element put into it, ' randomness ' in sense (1) is undoubtedly referred to ; it is assumed that we are ignorant of the physical causes which determine the machine's behaviour, but if they were known, its behaviour would be seen to be causally determined. And when it is further asserted that human behaviour, in so far as it manifests free-will, shows a random character, the implication is that our decisions may ultimately be explainable in terms of physics and physiology. Dr MacKay seems to suggest this : ' The suggestion is that the choices of a free man may likewise be governed by statistical distribution-functions which have a physiological representation and are in principle determinate ' (p. 120).

Dr. MacKay's parallelism between ' randomness ' in his artifice and free-will in a human being does not take us very far, since the two series of behavioural events may both exhibit ' randomness ', and yet in one case it may be due to deliberative activity, in the other the outcome of a complex interplay of physical causes. His whole account of ' randomness ' seems to be based on a failure to distinguish these two meanings ; meaning (2) does not necessarily entail meaning (1) ; to assume that it does begs the whole question of whether free-will may be thus represented.

W. Mays

HERBERT DINGLE AND DOUGLAS BUSH

Science and Literary Criticism

There is one aspect of Professor Douglas Bush's review of my book, *Science and Literary Criticism*, in the May issue of this *Journal*, on which I ask permission to comment, as it exposes me to a serious misunderstanding. The following are the relevant passages :

> ' He is acute . . . in discovering fallacies in the more or less scientific theories of some predecessors. . . . Although the last two [I. A. Richards and T. S. Eliot] have demanded precision in thought and language . . . Professor Dingle finds even them guilty of meaningless pseudo-statement and subjectivity '. Then some passages from my book are quoted, with the comment : ' Might not all this be called a chain of dogmatic and contradictory pseudo-statements ? . . . Professor Dingle, like most people, is more successful in criticising others' theories than in developing his own '. Another remark of mine is described as ' an unverifiable criterion of value which Dr Richards was censured for using '. (This presumably means, ' like one which Dr Richards was censured for using '). And again : ' Professor Dingle makes many suggestive general observations, though they hardly seem scientific '.

It would naturally be inferred from this that I had charged Eliot and Richards with a fault which I had then committed myself. What I wrote was this (pp. 31-32).

> I hope it is clear that I am not in the slightest degree blaming either Mr Eliot or Mr Richards for using words ' inexactly ' or making ' pseudo-statements.' If I have a controversy with them, it comes from their insufficient recognition or confession that they are doing so, and perhaps a little from my belief that, in these instances at any rate, they have given ' statements ' an unnecessarily wide berth. But in truth I do not want to quarrel, for they are too useful to me as witnesses. Here are critics who, as explicitly as one could wish, make known their intention to avoid ambiguity and to adhere to statements that have definite, objective meaning, and no sooner do they begin their job of criticising than they ' pseudo-state ' like the most Blakeish of poets. And the reason is that they cannot avoid it. There *are* no commonly accepted premises from which they can start, and try as they will, they cannot speak as though there were.

I thought that I would be understood as saying here that pseudo-statements were unavoidable in significant literary criticism, but on re-reading the passage carefully I see that I did not actually say so.

I said that *they* (Eliot and Richards) could not avoid such statements, there being no commonly accepted premises from which *they* could start—thus making it possible for a reader to infer that that was because of their weakness of intellect, and that I, being naturally a superior being, was about to show them how the thing could be done. This inference, it seems, Professor Bush has succeeded in making and he then proceeds to show that I have used pseudo-statements. In this, of course, he has no difficulty, though he has chosen examples which I would be prepared to defend as statements—certainly not dogmatic or contradictory when the essential steps in the argument, which he has deleted, are restored. He might have chosen many more convincing examples.

I greatly regret my obscurity of expression, not only because of the gross immodesty with which it appears to saddle me when analysed by so acute a critic as Professor Bush, but also because it makes me appear to imply exactly the opposite of the main conclusion of the book, which is that literary criticism is not a science. After all, the point is closely related to the philosophy of science which is my main intellectual interest, and I would not like readers of this *Journal* to think that I was trying to write of poems as though they were electrons.

HERBERT DINGLE

I am very sorry to have misrepresented Professor Dingle's attitude in any way; the fault was doubtless mainly mine, though my impression did not start from controversial prejudice but was gained through more than one reading of his book. However, when Professor Dingle says, in the comment above, that the main conclusion of his book was that literary criticism is not a science, I confess that my understanding of his whole argument is clouded. While he found defects in scientific approaches hitherto made, the *raison d'être* of the book seemed to be an effort to bring criticism closer to science. To cite only one of a number of passages, these are the last sentences before the three critical essays which he modestly offered as examples of ' Scientific Method.'

> A science of criticism, however, is not necessarily impossible, and might be created if, through progress in physiology or psychology, the requisite criteria should be found to exist. In the meantime, experience obtained in the prosecution of science might well be placed

at the disposal of critics. This would assist in preventing the formation of false theories of criticism, by application of the principle, brought most clearly to light in science, that phenomena are not 'explained' unless they are in some degree related with one another. It would also have constructive value in limited fields of criticism by introducing therein certain scientific methods, such as the use of hypotheses.

Douglas Bush

MATHEMATICAL LOGIC [1]

The last few years have seen a revival of interest in this country in a subject which may well be called a British invention and which certainly reached the first peak in its development here in Russell's *Principia Mathematica*, the study by means of a symbolic calculus of the laws of thought and the concepts and structures of mathematics. The cause of its neglect here between the wars, after so auspicious a beginning, seems to have been the very brilliance and success of the *Principia* itself, in much the same way that the study of the differential and integral calculus declined in this country two centuries earlier from a too slavish imitation of Newton's great work.

Russell himself was only too clearly aware of some of the shortcomings of the *Principia* ; the reflexive paradox which he had discovered at the turn of the century obliged him to complicate the *Principia* by introducing an elaborate theory of types to prohibit the formation of classes of mixed type and in particular to exclude the universal class. The theory of types in turn led to the introduction of the axiom of infinity, which asserted the existence of an infinity of objects, and the axiom of reducibility, which postulated the existence of a propositional function of lowest type with the same truth values as a given propositional function of any high type, and which was needed to validate Dedekind's construction of the least upper bound of a bounded set, a construction which leads to a bound of different type from the elements of the set. These axioms, with their appearance of making assumptions about the nature of the real world, brought the *Principia* into ill repute and the time was ripe for a new approach.

In England great ingenuity was expended in vain attempts to patch up the *Principia* but the ideas which were to dominate the development of the subject for the next quarter of a century came from outside this country. In Amsterdam L. E. J. Brouwer sought to found mathematics on ' the primary intuition of number ' and to guard it against the reflexive and allied paradoxes by a denial of the law of excluded middle ; this denial leads, however, to an even more drastic restriction of classical mathematics than the theory of types, as well as casting doubt upon *all* the laws of logic, and Brouwer sought to justify his proposal by showing that the rejected parts of mathematics, which roughly speaking comprised the body of pure existence proofs—that is, proofs which purported to establish the existence of some mathematical entity without providing the means for determining the object in question—were intrinsically unworthy of acceptance. Both Russell's logicism and Brouwer's intuitionism were opposed by Hilbert in Göttingen. Hilbert resolved to rid mathematics for all time of its foundations' troubles by expressing classical mathematics in a formal system susceptible of a proof of freedom from contradiction by methods entirely beyond reproach. Hilbert's formalism was said to be a calculus of signs, not of concepts, in which the laws of logic appeared as arbitrary conventions, or transformation rules of the signs, not as representations of universally valid laws of thought. The formalist programme was never completed and in 1931 Kurt Gödel showed it to be incapable of fulfilment by proving that every formal system which is rich enough to contain inductive arithmetic necessarily contains insoluble questions amongst which must be included the question of freedom from contradiction.

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[1] *Outlines of a Formalist Philosophy of Mathematics*, Haskell B. Curry, North-Holland Publishing Co., Amsterdam, 1951, Pp. vii + 75. *On the Mathematics of Algebra*, Abraham Robinson, North-Holland Publishing Co., Amsterdam, 1951, Pp. ix + 195.

Gödel's work inspired a new American School of mathematical philosophy to which Professor Curry, the author of one of the books under review, belongs. His *Outlines of a Formalist Philosophy of Mathematics* is the first in a new series of studies in logic and the foundations of mathematics which is being published in Amsterdam by the North-Holland Publishing Company and edited by Brouwer, Beth and Heyting. Other works in the same series include an account of Gödel's discoveries written by A. Mostowski, essays on modal logic by von Wright, B. Feys and J. C. C. Mckinsey, an account of many valued logics by J. B. Rosser and A. R. Turquette and also three books on late classical and mediaeval logic. Professor Curry has also contributed a volume on the algebra of logic to another new series of books on mathematical logic which is being produced by Gauthier-Villars. The simultaneous appearance of these two series devoted to mathematical logic is a strong indication of the present widespread interest in the subject.

The manuscript of Professor Curry's *Outlines* was written in 1939 so that the book represents, as he says, the views which he held then and not necessarily the views which he would defend today, but there can be little doubt that his present standpoint must be substantially that of the monograph or he would otherwise surely have hesitated to permit its publication.

The thesis of the book is that mathematics is the science of formal systems. The definition is an attractive one and if it does little else it serves well to characterise the author's standpoint. As a comprehensive definition of mathematics it is quite obviously unacceptable since the science of formal systems is no more the whole of mathematics than the metronome is the whole of music or linguistics literature. Composition, invention, definition, the discovery of the hidden connective and the unperceived difference, and above all *symbolism* are aspects of mathematics that lie outside formalism and give to mathematics its characteristic vigour and importance.

One of the commonest criticisms of a formalist account of mathematics is that the formalist fails to account for the applicability of mathematics. Professor Curry meets this criticism by saying that formal systems are to be judged by their acceptability, that is the extent to which they fulfil their ordained purpose. His position in this respect is an extreme one ; he holds, for instance, that so long as its usefulness persists classical analysis needs no other justification, and conversely that Brouwer's intuitionistic analysis is utterly useless because it is so complicated. It is hard to believe, in the light of past experience, that the intrinsic difficulty of any branch of mathematics is an absolute barrier to its application, and, in any case, difficulty is a relative notion and the difficulties of one generation are often the self-evident intuitions of another. Another weakness in the argument is that it ignores the existence of simpler finitist systems which may prove no less acceptable to modern physics than the classical theory of functions. The choice between finitist and non-finitist systems must therefore be made on other grounds than acceptability, and these grounds may be found in recent work on the interpretation of formal systems.

Whether the empirical formalism which it defends is acceptable or not, *Outlines of a Formalist Philosophy of Mathematics* is valuable for its analysis of the concept of a formal system and the steps which it takes to free the formalist foundations of mathematics from all philosophical preconceptions of the nature of mathematics.

Professor Robinson's *Metamathematics of Algebra*, on the other hand, is not principally a study of the foundations of mathematics but an attempt to find a new application for mathematical logic in the field of modern algebra. One of the first achievements of modern algebra was a formulation of the concepts of groups, rings and fields in general terms without regard to the numerical relationships out of which the ideas first arose. This may itself be regarded as an application of mathematical logic to algebra, and certainly the definitions of these concepts are greatly clarified by the use of a formal symbolism. Text-books on modern algebra so often mislead the uninitiated into thinking that group is a *class*, rather than a *relation* property. Instead of a clear idea that addition is a group property for integers, but multiplication is not, they are left to wonder whether the class of integers is itself a group, or if it is not. Obvious though the answer is to an expert it is not obvious to the beginner and a strict axiomatic formulation of the concept is, as Robinson shows, both easy to grasp and unlikely to be misunderstood. One of the primary objects of the *Metamathematics of Algebra* is to extend this abstract treatment to such concepts as *ideals*, *polynomial rings* and *algebraic varieties*. The appropriate definition of an ideal was first given by Tarski. Robinson expresses the definition in the following form. If K and J_0 are two sets of statements in a language L then a subset J of J_0 is called an *ideal* in J_0 over K if all the statements of J_0 which can be deduced from the totality of statements in J and K are included in J. Amongst the results obtained on these generalised ideals are a necessary and sufficient condition for an ideal to be irreducible, and a proof that every ideal is the meet of a finite number of irreducible ideals. The generalisation is shown to preserve the idea of a basis but the concepts of prime ideals and of primary ideals are said to be incapable of generalisation. This seems rather surprising in view of the results which Tarski has obtained on prime ideals in set theory (*Fundamenta Mathematica*, 32). One of the most important extensions of modern algebra which Robinson achieves appears to be his theory of disjunctive ideals, i.e. the ideals of a domain of statements which necessarily contains the disjunction of any two of its members.

The second direction in which the *Metamathematics of Algebra* applies mathematical logic to extend the methods of modern algebra is in the derivation of mathematical theorems from general propositions in symbolic logic, for example, from the readily provable proposition that there is no single statement in a formal language L which implies and is implied by all the statements in a monotonic increasing sequence of statements, i.e. a sequence of statements each of which is implied by (but is not equivalent to) its successor. By suitable specialisations of the sequence Robinson derives the theorems : any proposition of the restricted predicate calculus which is true for (*a*) all non-Archimedean ordered fields, (*b*) the field of all algebraic numbers, is true for all ordered fields and for any other algebraically closed field of characteristic 0, respectively. If a set of polynomial equations with integral coefficients has no solution in any extension of the field of rational numbers, then it has no solution in any field of characteristic $p > p_0$ where p_0 is a constant depending on the given set of polynomials and the coefficients are taken *modulo p*.

The author's claim to have enriched the potentialities of modern algebra by wedding it to symbolic logic seems to be amply substantiated.

R. L. GOODSTEIN

Sun, Stand Thou Still. The life & work of Copernicus the Astronomer, Angus Armitage, Sigma Books, Ltd., London, 1947. Pp. x + 210. 12s. 6d.

IN his popular version of his *Copernicus, the Founder of Modern Astronomy* Mr Armitage has given us a fascinating account of Copernicus and his times. The book is divided very suitably into three sections : astronomy before Copernicus—the man and his work—the triumph of the Copernican theory (a sketch of the transformation during the century following its publication). The author hopes that he has also provided an introduction to the history of astronomy in its wider aspects. He has certainly succeeded, and with its clear diagrams and charming plates, this work admirably fulfils the purpose for which it was written.

G. BURNISTON BROWN

Science and Civilization, edited by Robert C. Stauffer, University of Wisconsin Press, Madison, 1949. Pp. xvi + 212. $2.50.

THIS is a volume of papers on the history and philosophy of science given during one of the symposia held in celebration of the hundredth anniversary of the founding of the University of Wisconsin, and later published as a book. The symposium was organised by the History of Science Group of the University, which can boast two Chairs in the subject.

The volume opens with an evaluation by Professor Richard McKeon of the influence of Aristotle's scientific method on the origins of science in the West, an influence to which recent scholarship has given a more fruitful role than that which seventeenth-century polemics would have us accept. This is followed by a spirited account by Professor Lynn Thorndike of some examples of the intense empiricism of mediaeval science. Next comes a most interesting attempt by Professor Max Black to reach a 'persuasive definition' of scientific method. This is full of good sense and stimulating suggestions. He discusses the illusion of a method of automatic discovery; the fact that 'because the experimenter *interrogates nature*, every experiment is based upon a "preconceived idea," a hypothesis to be tested'; the importance, in different periods, of different extra-scientific ideas, such as a belief in determinism, as 'regulative principles' guiding research: these points he illustrates by an interesting analysis of Claude Bernard's *Introduction to the Study of Experimental Medicine*. Analysis of past method, he points out, may influence the present—surely one of the important reasons for studying the history of science: 'Principles abstracted from the past history may be put to work to help determine its future and themselves become subjected in this way to continuing experimental test. . . . Looking back at the history of science we choose such principles of method as will seem to answer best to the search for knowledge of the world—as in the light of that examination we feel constrained to envisage what we now call the world and knowledge of it.'

The fourth essay is an analysis by Professor Ernest Nagel of the meaning of 'reduction' in the natural sciences, especially that seen in the reduction of thermodynamics to statistical mechanics and the kinetic theory of matter. He shows that this is different from the reduction of Galileo's mechanics to Newton's. The first type of reduction he illustrates by a detailed discussion of the deduction of the Boyle-Charles Law from the general principles of mechanics, the kinetic theory, and some subsidiary assumptions. The essay concludes with a discussion of the problem of novelty, which, he points out, 'cannot be discussed without reference to the specific theory which formulates the nature of the elements and of their properties.'

Forming the last part of the book are essays by Mr Philip E. Le Corbeiller on 'Physics as a Cultural Force,' by Professor Farrington Daniels on 'Science as a Social Influence,' by Professor Owsei Temkin on the role of extra-biological metaphors (Galen's divine craftsmanship, Descartes' machine, Virchow's sociological metaphor of the cell state) in human biology, and by Professor William F. Ogburn on 'Science and Society.'

A. C. CROMBIE

The Art of Scientific Investigation, W. I. B. Beveridge, William Heinemann, London, 1950. Pp. xii + 171. 10s. 6d.

'PARADOXICAL as it may at first appear, the fact is that . . . scientific research is an art, not a science.' The apparent paradox in Professor Beveridge's conclusion is resolved in part when it becomes clear that he is concerned in this book more with the discovery and definition of problems than with matters of research design by which hypotheses derived from these problems may be put to test. Professor Beveridge has summarised his general point of view in this way : 'In this book I . . . have emphasised the limitations of reason as an instrument in making discoveries. . . Only the technicalities of research are "scientific" in the sense of being purely objective and rational.' He has '. . . tried to deduce and state simply as many guiding principles of research as possible.' These principles arise from his own experience and from reports of other scientists, past and present, whose successes in science are well established. In analysing these experiences he has been concerned '. . . with the psychology and practice of research.'

The book is written '. . . in the first place for the student about to engage in research . . .' and for such readers it will have its greatest value. Training of human behaviour as complex and little understood as creative thinking is at the present time largely dependent upon practice and upon guidance from those who have already been successful. The systematic study of creative work has only a short history, but descriptions of experiences during creative work have been recorded for a very long time, by artists and scientists, poets, novelists, mathematicians and physicists. Four stages in the process now appear to be well-recognised and are frequently labelled as : preparation, incubation, illumination and verification. It is during the last of these stages that the logic of scientific method is applied to research design and the techniques of the various sciences are employed in controlled observation and measurement. But it is during the first three stages that new problems are discovered and clarified. The precise nature of the processes involved in such discovery is not known, but a number of conditions favourable to discovery can be enumerated. For instance, Professor Beveridge refers to 'the process of intuition' as '. . . a clarifying idea that springs suddenly into the mind.' This description of the process does not help in understanding the basic nature of 'intuition.' The real value in

Professor Beveridge's discourse comes when he describes seven ' . . . conditions most conducive to intuitions . . . ,' conditions which may be put to practical application.

The ' principles ' which Professor Beveridge proposes vary widely in the level of behaviour at which they are aimed. In one place the reader is told that it is ' . . . useful to keep a card index with brief abstracts of articles of special interest . . . ', and, in another, that research workers ought deliberately to exploit chance occurrences. In the one case the advice is sufficiently explicit to be followed by any person capable of reaching the status of research apprenticeship. But in the other the advice would be useful only to individuals possessing the genius or experience, or both, which are basic to new discoveries. Recognition of the relations between chance occurrences and the nature of the basic inquiry and of the importance of exploiting them, involves most, if not all, of the essential processes of scientific discovery. Advice at this level is of value only in emphasising the importance of the initial stages in scientific investigation. In another instance the reader is informed that ' worry ' is one of the influences unfavourable to ' intuitions.' Recognising this may be important, but eliminating the ' worry ' may require much more fundamental ' principles.' Students are told that they need not fear making mistakes ' . . . provided they correct them in good time.' Certainly the basic advice for which the student is searching here is, first, how to recognise ' mistakes ' and, secondly, how to do so ' in good time.' Professor Beveridge's oscillation from very particular to very abstract advice may be disconcerting to the student, and even to a wider audience.

From many points of view the major weaknesses in this book centre round discussions of the nature of psychological processes underlying new discoveries. The author's treatment of ' imagination,' ' intuition,' ' reason,' ' scientific taste,' etc., do not enhance our knowledge of mental processes nor of the phenomena themselves. For instance, ' scientific taste,' ' . . . probably synonymous with " personal judgment " . . . ,' is described as having very important and widespread effects in scientific investigation. It may appear in man as an innate or ' endowed ' characteristic or it may ' . . . be cultivated by training oneself in the appreciation of science.' And, finally, it ' . . . derives from the summation of all that we have learnt from others, experienced or thought.' The concept is so general that the reader is perplexed as to what it does in fact refer and how he, student or expert, can begin to put it to use. There are many generalisations which will bother the reader if he be a student of human behaviour. For instance, would he agree, even if he knew precisely what the author meant, that ' . . . all learning is conditioning of the mind ' ? What is the evidence that curiosity, ' . . . as an incentive to thinking . . . ,' is instinctive and ' . . atrophies after childhood unless it is transferred to an " intellectual plane " ' ?

Numerous personality traits are attributed to the successful scientist. It is sometimes difficult to understand to just what the author is referring when he describes some of these traits, e.g. ‘ Conscious ignorance and intellectual honesty are important attributes for the research man.’ At other times the reader would like more information concerning means of developing or capitalising on such traits, e.g. ‘ the person attracted to research usually is one who retains more than usual of the instinct of curiosity.’ The author recognises that certain of the desirable traits do not differentiate the successful scientist from individuals successful in other human activities : ‘ A good intelligence, internal drive, willingness to work hard and tenacity of purpose are further prerequisites for success in research, as in nearly all walks of life.’ These criticisms of Professor Beveridge’s treatment of psychological processes characteristic of the successful scientist and his activities should not be allowed to undermine the value of this book. They should be taken as indicating that our understanding of such processes is far from adequate.

Whatever its weakness may be this text expresses a point of view regarding science and the training of young scientists which is of extreme importance, and too often forgotten. The real genius in science exerts of scientific logic to research design or of specific techniques to controlled its influence more in the discovery of problems than in the later application observation. Professor Beveridge had very commendable motives in writing this book ; he has provided much useful advice ; and he has recognised many basic problems in the ‘ art ’ of scientific investigation which warrant further systematic study.

R. W. Russell

A History of the Theories of Aether and Electricity : (The Classical Theories), Sir Edmund Whittaker, F.R.S., Thomas Nelson and Sons Ltd., Edinburgh, 1951. Pp. xiv + 434. 32s. 6d.

In 1910 Sir Edmund Whittaker published *A History of the Theories of Aether and Electricity from the age of Descartes to the close of the nineteenth century.* The original edition being exhausted, he has felt (so he writes in the Preface) that any new issue should describe the origins of relativity and quantum theory, and their development since 1900. To carry out this plan, he is engaged on a work of two volumes, of which the first is now before us. It is the book of 1910, to a considerable extent rewritten, with the incorporation of much additional material. There is also a change in the title ; the period covered by the volume is now indicated by the words *The Classical Theories.*

In what sense *classical ?* My dictionary tells me that, apart from its particular Graeco-Roman meaning, the word *classical* signifies *standard, first-class*, and while I would hate to have to rely on the Concise Oxford

Dictionary for the definition of *electricity* (peculiar condition of the molecules of a body or of the ether surrounding them, developed by friction . . .), I think that the dictionary's definition of *classical* is in accordance with common usage. I give it as my opinion that Sir Edmund Whittaker's book is *classical*, and will be so regarded for many generations ; but as for the theories described in it, I would hesitate to assign the word *classical* to them in a lump like this, particularly as this sub-title to the first volume implies that what we shall find in the second volume will not be classical—and if anything deserves to be called classical, surely the Special Theory of Relativity does !

Any physicist who has read this review so far will wonder where I have hidden my head for the last quarter of a century. Do I not know that *classical* in physics now means *pertaining to the past*, *old-fashioned*, *outmoded* (except as an approximation to reality, approaching reality only under certain limiting circumstances, such as smallness of velocity or greatness of mass) ? Yes, I know the usage only too well. But while admitting that we can and must coin new words and give old words new meanings in physics, I deplore unnecessary linguistic debauchery for which we have to pay later in confusion of thought. I think it a pity that Sir Edmund Whittaker has allowed himself to be seduced by the prevailing jargon, for his book will be read long after the current slang has been forgotten.

In this book the reader follows the history of optics, electricity and magnetism (with excursions into gravitation and thermodynamics) through a period of some three hundred years. It is a history of concepts about nature, born in mysterious ways in the minds of extraordinarily gifted men and subjected to the constant test of experiment. I suggest that this history might be regarded as the history of *words*. For the concept in the individual brain and the experiment performed in private mean little to physics until they are communicated, and for communication in physics there are only three channels : (*a*) take the other man into your laboratory and show him by gestures the experiment in progress, (*b*) write or speak words, and (*c*) write mathematical formulae. As far as theoretical physics is concerned, (*a*) does not apply, and mathematical formulae are for the most part treated only as bridges between words. In the last analysis, it is the *word* that counts.

Some words are good and some are bad, some survive and some die out. A word is good in theoretical physics when it corresponds crisply to something which (with superhuman powers) one can imagine oneself looking at or handling, or to some mathematical formulation (the simpler the better). A word is bad when the correspondence is poor, or when at its mention a vague cloud rises in the mind. These ideas about good words and bad are not taken directly from the book, but they are suggested by it, for the author is very sensitive to words and has been at great pains to give the sources of the key-words in physics. Unfortunately the publishers have committed the unpardonable sin of allowing a book of such importance to appear without a subject-index (a table of contents, though full, is not enough), and there is much fluttering of pages to recover delightful tit-bits of information, often contained in footnotes. Here is one of them : '*Kinetic energy* was first used in an article by W. Thomson and P. G. Tait in *Good Words* (then edited by Charles Dickens), October, 1862.'

In his Preface, Sir Edmund Whittaker pleads for the rehabilitation of the word *aether*. It is painful to have to go against him, but I feel that I must, because *aether* is a bad word. Not bad in the title of his book, of course, because the book deals with attempts to use the aether (I follow Sir Edmund's spelling in the face of the Oxford Dictionary) as the basis of optical and electromagnetic theories. But for the present and the future it is a bad word, because it suggests, and its function in the nineteenth century was to suggest, a continuum of points individually identifiable for all time. Otherwise a *vibration of the aether* would mean nothing. But that is not all. We should beware in physics of words which excite philosophers—words with a sort of irridescent aura. *Wave-front*, *ion*, *kinetic energy*, *electron*—these are good words and professional philosophers do not prick up their ears at their mention. It is otherwise with *aether* and *relativity*, and Minkowski did well to describe the latter word as feeble when he displayed Einstein's theory in classical simplicity in terms of the geometry of space-time (a good word). Sir Arthur Eddington (*New Pathways in Science*, Cambridge University Press, 1935, p. 39) and recently Professor Dirac (*Nature*, 1951, **168,** 906) have also given their support to *aether*, so I am being very pig-headed—but I still think that *aether* is a bad word.

This *History* is backed by a vast erudition, but it is never overpowering, for the style is sprightly and the author is singularly successful in putting himself and the reader in the place of each physicist whose work he is reconstructing. He manages with great skill to mingle the atmosphere of contemporary confusion which always accompanies scientific progress with an appreciation of what was actually going on, as viewed in the light of later knowledge. We see the net being woven mesh by mesh, and, if we did not know otherwise, we would expect the second volume to tell of the catching, cooking and eating of the fish. We shall await with a lively expectation Sir Edmund Whittaker's handling of the slippery things that did come on board.

There are thirteen chapters in the book. The first tells of theories of the aether up to the death of Newton. The second discusses early work in electrostatics and magnetism, bringing out the fact, perhaps not well enough known, that the inverse square law in electrostatics (commonly called Coulomb's) is rightly Priestley's. The law for magnetism is Michell's. The third chapter deals with galvanism (Galvani to Ohm), and the fourth takes us back to the luminiferous aether (Bradley to Fresnel). In the fifth chapter the aether as an elastic solid is treated in a very attractive way. It is here that the partial differential equation (the essence of modern physics) begins to show its power. Chapters VI to VIII carry us from the intuition of Faraday to the mathematics of Maxwell, and then we read of the determined attempts of Kelvin, FitzGerald and others to construct mechanical models of the aether (the vortex-sponge sounds quaint to modern ears) and of the experiments of Hughes and Hertz which validated Maxwell's guess as to the electromagnetic nature of light. Chapter XI tells of conduction in solutions and gases, culminating with the discovery of the electron by Sir J. J. Thomson ; the last two chapters deal with ' classical ' radiation-theory and ' classical ' theory in the age of Lorentz (reviewer's quotation marks !).

In reference to Francis Bacon and Galileo Galilei, as set against the background of the Middle Ages, Sir Edmund Whittaker writes : ' They rejected altogether the Aristotelian practice of referring everything in physics to Final Causes, and asked *How ?* rather than *Why ?* ' In this book he has told us *how* men from small beginnings created in three centuries a vast coherent body of knowledge. The application of this knowledge to the comfort, convenience and general material well-being of mankind all over the world is a commonplace of modern life—to be electrified is to be civilised. But as long as men are men, the *Why ?* will not be suppressed. We may not ask *why* all ultimate electric charges are the same in magnitude, but we have an itch to ask *why* so many men of genius devoted themselves so intensely to uncover nature's secrets in these particular fields. Marxists find the answer in economic causes. I see no shred of evidence in this book that they are right. But, if the Marxists are wrong, what is the answer ?

J. L. Synge

Essay in Physics, Viscount Samuel, Basil Blackwell, Oxford, 1951. Pp. vi + 154. 7s. 6d.

It is exceptional for an author to publish an authoritative review together with his thesis. The procedure has so much to commend it that this reviewer hastens to take advantage of it. Lord Samuel includes in his book a letter from Dr Albert Einstein which itself provides the most effective commentary on the basic theme.

Dr Einstein reflects, one would surmise with sympathetic patience, that Lord Samuel is looking to physics to describe what is ' physically real.' The latter seems to suggest that physicists cannot hope to reach their goals by the employment of purely fictitious concepts such as number—and that ' contemporary physics are based on concepts somewhat analogous to the " smile of the absent cat ".'

Dr Einstein's answer is, of course, that it is merely the data of our consciousness which are given to us and that 'there is only one way from the data of consciousness to "reality," to wit, the way of conscious or unconscious intellectual construction.' In other words, all our ideas of 'objects' are 'constructs,' and our confidence in them is based on the fact that they stand in a relation of 'correspondence' with our sensations. To say, with Lord Samuel, that the real world exists independent of our mental constructs, has for Dr Eintsein no other meaning than 'I *believe* that there exists a satisfactory theory based on the assumption of fictitious objects extended in space-time and their regular relations.' It becomes a matter of intuition to pass, with Lord Samuel, to the further conclusion that such a theory might be so developed as to give a complete description of reality, the laws of which establish relations between the things themselves.

This friendly exchange will perhaps serve to indicate the broad tenor of Lord Samuel's 'realist' challenge on behalf of the lay belief that the physicist's concepts are 'mere ideas' as contrasted with the 'real' objects of daily experience, ignoring the fact that the latter just as much as the former are 'constructs' derived from the data given in consciousness.

He has a similar uneasiness as to the activities of the mathematicians and argues for a non-mathematical approach in the province where philosophy and physics meet and which cannot, in present knowledge, seek or expect mathematical support. In support of this, he asks how can mathematics claim to be more than a mental treatment of symbolic measurements of what science calls 'the observables?' How impossible it is to conclude with science that 'unobservables' have no significance for human understanding! 'It is to the realm beyond the range of scientific observation that we may now be well-advised to devote our special attention.'

The challenge of Lord Samuel's philosophic speculation thus passes to a new essay in 'realistic' construction. Newton said '*hypotheses non fingo*,' yet himself went on to advance theories which postulated an 'ether' throughout space. From this, Lord Samuel traces the scientific need for an 'ether' and points out that the space-time continuum of Relativity is really an alternative name for an ether. 'The fact is that the old fixed notion of ether collapsed because it had materialistic connotations whereas the new belief in the need for an ether does not require any such materialistic implications.' There is, for example, the need to explain:

(i) the nature of electromagnetic radiation and the medium for its transmission;
(ii) the nature of gravitation and its propagation; and
(iii) the nature of motion and momentum.

Such problems necessitate a new notion of an ether which will reconcile the basic problems of modern physics; these hinge on the notion that electrons have both a corpuscular aspect and an undulatory aspect. That is to say that energy changes or transmissions are manifested either as waves or as particles. The two concepts appear contradictory but ' yet physics has to stumble along and work with them both.'

Lord Samuel breaks through with a new proposition which he clearly offers only as a tentative approach. For him there must be an ' ether ' which may well be continuum of energy—and energy can change from a state of quiescence to a state of activity or *vice versa.* ' Quiescent energy is conceived as a continuum, and as the sole physical constituent of the universe. All material events are to be accounted as cases of the activation of quiescent energy.'—There is postulated a ' two-state ether ' which is basically in the quiescent state but is subject to activation manifested either as waves propagated spherically or as particles in linear projection. ' Matter is clotted ether.'

The system has to be accepted as an existing fact. It is impracticable to pursue the infinite regress and explain how this two-state ether came into existence ; we can but observe its activation as the vehicle of the processes of the physical universe. The conception would seem to be that the physical universe as a whole moves through its processes with variations corresponding to the theme of its ' melody ; ' within this setting, Reality holds its mental and physical counterparts in reciprocal interaction.

For Lord Samuel, the hypothesis remains of an all-embracing universe which we picture as ' an ocean of energy in a state generally of quiescence, but with waves of radiation constantly flickering in it ; and numberless particles forming and either enduring or dissolving.' His treatment of space and time is in line with Kantian phenomenalism but now the emphasis is not on space and time as individual aspects, but on the space-time continuum itself as a mental interpretation of the two-state ether. He concedes the basic fact of causation but yet asserts that freedom of will can be maintained in control of it. Thus we get a new attempt to give a working hypothesis reconciling the difficulties which physics itself is believed to be unable to explain.

The *Essay* gives vent to the typical desire of everyday life for some ' realistic ' basis to a universe of ' things ' bound in unceasing reciprocity of activities. It springs in fact from a sub-conscious and growing revolt over the last fifty years against the failure of science to bring its own philosophy to clear expression. The situation has been aggravated by the omission of popular scientific expositors to warn their readers against ' realistic ' interpretations of their presentational extravagances or to explain the symbolic character of the facile concepts they so readily employ.

In a rejoinder to criticisms in the current issue of *Philosophy*, Lord Samuel re-presents his basic emphasis on the distinction between (*a*) the events and processes of nature, (*b*) sense-apprehensions of those events and processes, and (*c*) subjective and mental ' constructs.' He re-offers his speculative 'energy' continuum for the consideration of physicists; and so the wheel must go full circle again: Lord Samuel's essay into physics is brave but can only claim serious consideration if it survives critical examination by the mathematicians. They are the custodians of the mental 'drawing board' where the pioneering speculations of the philosopher must be vetted by the physicist.

HENRY SELF

Natural Science and the Spiritual Life, being the Philosophical Discourse delivered before the British Association for the Advancement of Science at Edinburgh on 12th August 1951, by John Baillie, Oxford University Press, 1951. Pp. 43. 5s.

THE main point of this address is to show how some of the principal characteristics of modern science, as distinct from ancient Greek science, developed as a result of certain doctrines belonging to Christian theology; these are presented as part of a philosophical outlook held to do violence neither to science nor to human experience.

The basic presupposition of ancient Greek science was that observed events were the product of 'essences' discoverable by scientific inquiry. There was no sharp distinction between God and nature; the physical world might be an emanation from God but it was of the same essential nature as God. Scientific procedure was aimed at penetrating, by a sort of intellectual intuition, to knowledge of the essences of things; explanation meant showing deductively how observed behaviour followed from that. Thus the knowledge gained by natural science was ontological, indeed theological knowledge of a deterministic system of cause and effect which, once accepted, allowed for no further inquiry or speculation. Events could happen in no other way.

In contrast with this, theories in modern science are held to be descriptions (albeit sophisticated ones) of observations, and not to be statements about the essential nature of things. They are essentially tentative, held only so long as they survive experimental tests; scientific knowledge grows through the new experiments suggested by theories, experiments that may lead theories to be replaced by more accurate ones. This modern conception of science Professor Baillie, following Mr Michael Foster and Whitehead, attributes mainly to the Christian (and Hebrew) doctrine of creation. God was no longer confused with nature. All things were indeed ordered by divine wisdom, but as a free creation of the divine Will, not a determined emanation from the divine Reason. Providence was not in doubt, but was inscrutable to scientific inquiry. Accepting the physical world as contingent and given, the only way of discovering anything about it was by observation, hypothesis and experiment. No longer commanding respect as being ‘ full of gods,’ nature could now be conceived of in any way that conduced to scientific convenience and control.

The thesis described by Professor Baillie is interesting and plausible, and would certainly repay further historical and philosophical inquiry. A few observations suggest themselves. The descriptive view of certain types of scientific theory (those used to describe planetary motions) was held by the pagan Greek astronomer Ptolemy and the pagan philosopher Simplicius. It was revived in the thirteenth century in Oxford and Paris as a direct consequence of Latin translations of Simplicius' writings. In the fourteenth century, William of Ockham applied it to scientific theories in general. But in the seventeenth century there seems to have been a revival of the ‘ essentialist ’ view. Galileo devoted his major scientific effort to trying to prove that the Copernican system was ‘ true ’ ; the anti-Galileo theological party were motivated, at least partly, by the fear that he might succeed (and thereby contradict Biblical texts), although many contemporary theologians held the descriptive view of scientific theories and regarded the whole controversy as unfortunate. The essentialist view, held by the scientists themselves, was again at the heart of the controversies over geology in the early nineteenth century and, later, of those over Darwinism. Indeed, both views have been widely held by scientists and philosophers from the beginnings of modern science in Christian Europe in the thirteenth century down to the present day. Outside the fold of Christendom, the ‘ essentialist ’ view is now being pressed, ostensively in the interests of science itself (or should we say of ‘ scientism ? ’), in those areas of the world dominated by Marxism. It has long been obvious that the study of the history of science needs to take account of opinions held for non-scientific as well as for scientific reasons. Professor Baillie's lecture may well release a train of thought and reading in a sympathetic historian. But whatever it is that results, it will certainly not be simple.

A. C. Crombie

RECENT PUBLICATIONS ON THE PHILOSOPHY OF SCIENCE

(*a*) Books Received for Review

Inclusion of books in this list does not preclude their being reviewed in later issues

L. von Bertalanffy, *Problems of Life—An Evaluation of Modern Biological Thought*, Watts & Co., London, 1952 (German Original: *Das Biologische Weltbild*, A. Francke-A. G. Bern, 1949), pp. ix + 216, 25s.

Philotheus Boehner, *Medieval Logic*, Manchester University Press, Manchester, 1952, pp. xvii + 130, 12s. 6d.

Sir Russell Brain, *The Contribution of Medicine to our Idea of the Mind*, The Rede Lecture for 1952, Cambridge University Press, 1952, pp. 30, 2s. 6d.

P. W. Bridgman, *The Nature of Some of our Physical Concepts*, Philosophical Library, New York, 1952, pp. 64, $2.75.

Herbert Dingle, *The Scientific Adventure*, Sir Isaac Pitman & Sons, Ltd., London, 1952, pp. ix + 372, 30s.

Hugo Dingler, *Das Physikalische Weltbild*, Westkulturverlag Anton Hain, Meisenheim/Glan, 1951, pp. 56, DM. 4.10.

Lazarus Ercker, *Treatise on Ores and Assaying*, translated by Anneliese G. Sisco and Cyril Stanley Smith, University of Chicago Press (London: Cambridge University Press), 1952, pp. xxxiii + 360, 75s.

Ernesto Grassi und Thure von Uexküll, *Von Ursprung und Grenzen der Geisteswissenschaften und Naturwissenschaften*, Verlag A. Francke-A. G. Bern, 1950, p.p 252, Fr. 12.60.

Francis L. K. Hsu, *Religion, Science and Human Crises*, Routledge and Kegan Paul, Ltd., London, 1952, pp. vii + 142, 14s.

Olan R. Hyndman, *The Origin of Life and the Evolution of Living Things*, Philosophical Library, New York, 1932, pp. xxi + 648, $8.75.

Konrad Z. Lorenz, *King Solomon's Ring*, Foreword by Julian Huxley, Methuen & Co. Ltd., London, 1952, Translation from German by Marjorie Kerr Wilson, pp. xix + 202, 15s.

Edward MacCurdy, *The Mind of Leonardo da Vinci*, Jonathan Cape, Ltd., London, first published 1928, reissued in the Life and Letters Series, 1932, reissued 1952, pp. 360, 18s.

Bertrand Russell, *Dictionary of Mind, Matter and Morals*, Philosophical Library, New York, 1952, pp. xiv + 290, $5.00.

A. Speiser, *Elemente der Philosophie und der Mathematik*, Verlag Birkhäuser, Basel, 1952, pp. 115, Fr. 11.45.

F. Sherwood Taylor, *The Alchemists*, William Heinemann, Ltd., London, 1951, pp. x + 246, 12s. 6d.

N. Tinbergen, *The Study of Instinct*, Clarendon Press, Oxford, 1951, pp. xii + 228, 25s.

Gerhard Tintner, *Econometrics*, John Wiley & Sons, Inc., New York (London: Chapman and Hall), 1952, pp. xiii + 370, 46s.

Transactions of the Conference of Morale—and the Prevention and Control of Panic, New York Academy of Medicine and the Josiah Macey Jr. Foundation, pp. iv + 75.

RECENT PUBLICATIONS

(*b*) Journals

The contents of the latest issues of Mind *and of the* Journal of the History of Ideas *are printed with the advertisements on the cover of this issue*

Contributions to the Analysis and Synthesis of Knowledge (*Proceedings of the American Academy of Arts and Sciences*, Boston, Mass., U.S.A., 1951, **80**, 1-112) :

Philipp Frank, ' The logical and sociological aspects of science '
W. H. Sheldon, ' Integration in the biological and social sciences '
Charles Morris, ' The science of man and unified science '
Hans Reichenbach, ' The verifiability theory of meaning '
Carl G. Hempel, ' The concept of cognitive significance : a reconsideration '
Gustav Bergmann, ' Comments on Professor Hempel's paper '
John Lotz, ' Natural and scientific language '
W. V. Quine, ' Semantics and abstract objects '
Max Black, ' Comments on the paper of Professor Quine '
Alonzo Church, ' The need for abstract entities in semantic analysis '

Dialectica (Zürich, Switzerland), 1951, **5**, No. 3/4 : special number on 'Le Dynamisme psychique (psychodynamics),' edited by Jean Paulus :

J. Paulus, Editorial
A. Alexander, ' Three fundamental dynamic principles of the mental apparatus and of the behaviour of living organisms '
T. M. French, ' The integrative process '
A. H. Maslow, ' Higher needs and personality '
H. A. Murray, ' Some basic psychological assumptions and conceptions '
S. Rosenzweig, ' Idiodynamics in personality theory with specific reference to projective methods '
C. Buckler, ' Maturation and motivation '
F. Duyckaerts, ' L'anthropologie d'Oswald Schwarz '
E. De Greeff, ' Les modes de rattachements instinctifs, fonctions incorruptibles '
J. Paulus, ' Philosophy of human life, some of the conditions of a happy maturity and old age '
H. Wallon, ' L'Évolution dialectique de la personalité '
J. Delay, ' Ribot et le Jacksonisme '
V. Fontes, ' Le psychisme et les structures anatomiques '

The Journal of Symbolic Logic (Baltimore, U.S.A.), March 1952, **17**, No. 1 :

Andrzej Mostowski, ' On direct products of theories '
Alan Rose, ' An extension of computational logic '
Haskell B. Curry, ' The system L D '
G. Kreisel, ' On the interpretation of non-finitist proofs—Part II '

Methodos (Milan, Italy), 1951, **3**, section of methodology :

Dalberto Faggiani ' Sulla struttura della fisica '
Gerhard Frey, ' Das Residuum der natürlichen Sprache'
John Freund, ' On the problem of confirmation '
Juhos von Béla, ' Die Anwendung der logistischen Analyse auf philosophische Probleme '

Abraham Kaplan, Hermann F. Schott, ' A Calculus for empirical classes '
Louis O. Kattsoff, ' Reichenbach's treatment of " existence " in his logic '
Paul Lorenzen, ' Ueber das Prinzip " ex falso quodlibet " '
Gabriël F. N. Nuchelmans, ' Notes on the method of convergence '
Vittorio Somenzi, ' Fisica e geometria '
Thomas Storer, ' An analysis of logical positivism '

1952, **4** :
Silvio Ceccato, ' Contra Dingler, pro Dingler '
Charles Davenport, ' The role of graphical methods in the history of logic '
Dalberto Faggiani, ' Sulle proposizioni primitive della fisica '
Samuel Issmann, ' Problèmes de la definition '
Juhos von Béla, ' Die " Wahrheit " wissenschaftlicher Sätze und die Methoden ihrer Bestimmung '
Allende Luciano Lezama, ' La Unitad de la filosofia y de la filisofia y ciencia en una metodologia universal '
Paul Linke, ' Eigentliche und uneigentliche Logic '
Jean Piaget, ' La logistique axiomatique ou " pure ", la logistique operadoire ou psichologique et les réalités aux quelles elles correspondent '
Herbert Sanborn, ' Dingler's methodical philosophy '
Giuseppe Vaccarino, ' Consapevolezza del formalismo '

Philosophy of Science (Baltimore, U.S.A.), January 1952, **19**, No. 1 :
Hans Reichenbach, ' The syllogism revised '
Frank E. Hartung, ' Problems of the sociology of knowledge '
Adrian C. Moulyn, ' Time in relation to neurophysiology and psychology '
Erwin Biser, ' Postulates for physical time '
C. W. Berenda, ' On verifiability, simplicity, and equivalence '
Thomas Storer, ' Linguistic isomorphisms '

April 1952, **19**, 2 :
Oliver L. Reiser, ' The evolution of cosmologies '
Milton K. Munitz, ' Scientific method in cosmology '
Lester S. King, ' Is medicine an exact science ? '
Adrian C. Moulyn, ' The functions of point and line in time measuring operations '
John Somerville, ' A key problem of current political philosophy : The issue of force and violence '
Horace S. Fries, ' Is the ψ-function description " complete "? A layman's question '
William H. Hay, ' Professor Carnap and probability '

Theoria (Lund, Sweden), 1951, **17**. *A Philosophical Miscellany presented to Einar Tegen :*
Gunnar Boalt, ' Family Name and Social Class '
C. D. Broad, ' Locke's doctrine of substantial identity and diversity '
Edmund Dahlström, ' Some aspects of quantitative verification in sociology '
Martin Fries, ' Zur Interpretation einger Gedanken in der Philosophie Hägerströms '
Sören Halldén, ' What is a word ? '

Ingemar Hedenius, 'A note to Plato's Republic X, 596 c-d'
Torsten Husén, 'The influence of schooling upon IQ'
David Katz, 'Zwei Beiträge zur Psychologie der Wahrnehmung. I. Der Augenkontakt. II. Transponierung optischer Gestalten durch Raumverdichtung'
Olof Kimberg, 'La prison facteur criminogène'
Rolf Lagerborg, 'L'Ethical relativity de Westermarck'
Andries H. D. MacLeod, 'Implikation und Konsequenz nach Gunnar Oxanstierna'
Konrad Marc-Wogau, 'Kants Lehre von analytischen Urteil'
Harold Morin, 'Wille und Zeit in Schopenhauers Philosophie'
Manfred Moritz, 'Über den Satz " die Handlung H ist Pflicht'
Åke Pfannenstill, 'A critical analysis of operational definitions'
Hans Regnell, 'On negation and negative facts'
Ivar Segelberg, 'The intentionality of gladness'
Torgny T. Segerstedt, 'Some assumptions in attitude research'
Lajos Székely, 'Die Realität in der Auffassung Freuds'
Anders Wedberg, 'Some problems in the logical analysis of legal science'
Sven Wermlund, 'Culture, behaviour, and patterns of behaviour'

(*c*) Articles

Robert E Bass, 'Unity of nature,' *Human Biology*, 1951, **23**, 323-327

Ludwig von Bertalanffy, 'Goethe's concept of nature,' *Main Currents in Modern Thought*, **8**, No. 3

'Theoretical models in biology and psychology,' *Journal of Personality*, 1951, **20**, 24-38

'Problems of general system theory,' *Human Biology*, 1951, **23**, 302-312

'Conclusion,' *Human Biology*, 1951, **23**, 336-415

'Towards a physical theory of organic teleology,' *Human Biology*, 1951, **23**, 346-361

Magdalena Bleyle, 'Introducción a una nueva metodologia del conocimiento,' *Boletín especial de la Associación Argentina de Epistemologia*, April 1952, No. 3

A. M. Dalcq, 'Le problème de l'évolution est-il pres d'être resolu ?,' *Annales de la société royale zoologique de Belgique*, 1951, **82**, 117

'Mobiles et raisonnements dans l'exploration de la morphogénèse,' *XV Congrès international de philosophie des sciences* (Actualités scientifiques et industrielles, No. 1155) Hermann et Cie, Paris, 1949

Carl G. Hempel, 'General system theory and the unity of science,' *Human Biology*, 1951, **23**, 313-322

Hans Jonas, 'Comment on general system theory,' *Human Biology*, 1951, **23**, 328-335

P. T. Landsberg, 'The uncertainty principle as a problem in philosophy,' *Mind*, 1947, **56**, 250-256

Mario Lins, 'The cleavages of the classical sociological theory' *Third Congress on General Semantics*, Chicago, 1948 (*Jornal do Commercio*, 1952)

'The crisis of the sociological systematics,' *Fourteenth International Congress of Sociology*, Rome, Italy, 1950 (*Jornal do Commercio*, 1951)

Lancelot Law Whyte, 'Time and the mind-body problem. A changed scientific conception of process,' *Eranos-Jahrbuch XX*, Rhein-Verlag, Zürich, 1952

ANNOUNCEMENTS

The *Seventh International Congress of Linguistics* will be held at Birkbeck College (University of London), Malet Street, London, W.C.1, on 1-6 September 1952. Inquiries should be sent to the Honorary Secretary of the Executive Committee, Mr. D. M. Jones, at Birkbeck College.

The *Seventh International Congress for the History of Science,* under the auspices of the International Academy and the International Union for the History of Science, will take place in Jerusalem on 4-12 August 1953. The Congress will be under the patronage of the President and the Prime Minister of Israel. Among the subjects discussed at the Congress will be 'Relations and cultural exchange between East and West', and 'Certain problems of the history of technology, including the history of naval architecture'. Members are free to submit papers on any aspect of the history of science. All inquiries should be addressed to the President of the Congress, Professor F. S. Bodenheimer, c/o Hebrew University, Jerusalem, Israel.

The *Eleventh International Congress of Philosophy* will take place in Brussels on 20-26 August 1953. The Committee intend to arrange special sessions devoted to the following subjects : Experience and Metaphysics, The Force of Proof, Explanation in the Natural Sciences, Knowledge of Other People, Signification, The Intelligibility of History, The Foundation and Limits of Authority, The Incidence of Relativism on Moral Obligation, Robert of Lincoln, called Grosseteste (died 1253), George Berkeley (died 1753). All inquiries should be addressed to the Secretary, M. Ch. Perelman, 32 rue de la Pêcherie, Uccle-Bruxelles, Belgium.